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TRANSACTIONS

OF THE

CONGRESS

OF

47142

American Physicians and Surgeons.

THIRD TRIENNIAL SESSION,

HELD AT

WASHINGTON, D. C.

May 29th, 30th, 31st and June 1st, 1894.

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At a meeting of the Executive Committee held March 17th, 1893, the following resolution was adopted, viz :—

"It was moved by Dr. Loomis and seconded by Dr. Taylor, in place of the arrangement of former Congresses, that three afternoons and one evening of the Third Congress be given, in sessions of one and one-half hours each to seven of the fourteen participating organizations; and that each Society selected to perform this duty prepare its own programme for the period assigned to it by the Committee and select its own Speakers."

The selection of the Societies under this resolution to prepare programmes was made alphabetically as follows, viz :—

1st. The Association of American Anatomists.
2d. The American Climatological Association.
3d. The American Dermatological Association.
4th. The American Association of Genito-Urinary Surgeons.
5th. The American Gynecological Society.
6th. The American Laryngological Association.
7th. The American Neurological Association.

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MINUTES.

The third triennial session of the CONGRESS OF AMERICAN PHYSICIANS AND SURGEONS took place in Washington, D. C., on May 29th, 30th, 31st and June 1st, 1894.

The first meeting was called to order at 3 P. M., Tuesday, May 29th in Metzerott's Music Hall, corner of Twelfth and F streets, by Dr. Landon Carter Gray, Chairman of the Executive Committee. He said :—

I think that we may congratulate ourselves at this, the third meeting of the Congress of American Physicians and Surgeons. I can remember very well when, some seven or eight years ago, there were those of us who went into this triennial gathering with misgivings, mainly, I believe, because we feared some infringement upon the autonomy of our constituent associations. To our eyes at that time dangers since proven to be imaginary loomed up so large as to pale such advantages inherent to the touching-at-elbows of so many earnest men as are evidenced by the great diversity of interests in our programmes for the next few days. We are not met for merry-making and junketing, but for the interchange of thought and the observation sprung of thought. It is not extravagant to say that each Association fairly represents the leading minds in that branch of our art; so that the Congress puts before you the flower of American Medicine. The stimulus of such a coming-together is incalculable; for just as there are *insanities* that spread by mental contagion, so there are *sanities* communicable in the same way. Since we are prone to regard the great men as all dead, unless they live at a distance, and since we are much more facile in detecting a great man when his career has had exclamation points put to it by the grave than we are in recognizing that same great man when he stands before us in the garb, the passions, and the shortcomings of his age, we are likely to forget that there may be men in this Congress whose names will not die with them. I am not one of those Americans who hold that patriotism consists of unduly clamoring for applause for everything native; and yet I cannot help feeling that the tendencies represented in this Con-

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gress are those of the most advanced medical thought of our day. How great this advance has been we do not always stop to realize in our busy lives. After the death, in 1735 of Boerhaave, the great Dutch physician, and the foremost of the eighteenth century, to whom the city of Leyden erected a monument with the simple, affectionate, but significant inscription: "Salutifero Boerhaavii genio sacrum" (Sacred to the health-giving genius of Boerhaave), it was found that he had left a book whose title page stated that it contained all the secrets of medicine. All its pages were blank except one, and on that was written: "Keep the head cool, the feet warm, and the bowels open." Here in this city of Washington, 159 years after, there are convened fourteen different associations of men whose lives have been largely or entirely given to the study and practice of fourteen different branches of medicine. What a vast intellectual gulf has there been bridged! On the one side the eager mind of genius, baffled by the darkness. On the other the smiling dawn of knowledge, illumining the fogs of the centuries!

It now becomes my duty to present to you a gentleman who needs no introduction at my hands, Dr. Alfred L. Loomis, of New York, the President of this Congress.

The President, Dr. Alfred L. Loomis, of New York said:

GENTLEMEN—I thank you for the honor conferred upon me by your Executive Committee in electing me President of this Congress. The Congress of American Physicians and Surgeons is a representative body of the best thought and work of the medical profession in this country and we are all proud to be included in its membership.

The programme which has been prepared by your executive committee for this meeting is certainly most promising, and the time which has been allotted for the work before us seems far too short for its accomplishment. I shall, therefore, not detain you with any unnecessary words of congratulation, but will proceed immediately with the business of the Congress.

The chairman of the Committee of Arrangements, Dr. Samuel S. Adams, of Washington, D. C., then announced the place of Registration to be at the Arlington Hotel, gave information as to the method, the status of visitors to the Congress, and of the Reception of the PRESIDENT, on Thursday evening, May 31st, at 8 o'clock.

The President then announced the first meeting of the Congress to be under the auspices of THE ASSOCIATION OF AMERICAN ANATOMISTS and introduced Prof. Harrison Allen, of the University of Pennsylvania, to read a paper on "Morphology as a factor in the Study of

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Disease" (p. 1). After the reading of Prof. Allen's paper Dr. Thomas Dwight, of Harvard Medical School (p. 15), Dr. Frank Baker, of the University of Georgetown (p. 17) read papers in discussion of the subject and Dr. Burt G. Wilder, of Cornell University, spoke (p. 23). Dr. Wilder having in the course of the discussion stated that he had not had time to prepare a written paper on a case that he had wished to present, it was voted that he be requested to furnish a report for publication in their Transactions (vide appendix p. 344). The discussion was closed by Dr. Allen (p. 26).

The second meeting of the Congress was opened on Wednesday, May 30th, at 2 P. M., under the direction of THE AMERICAN CLIMATOLOGICAL ASSOCIATION. The subject was "Sewer Gas." The first paper was by Dr. Alexander C. Abbott, of the University of Pennsylvania, entitled "Chemical, Physical and Bacteriological Studies upon Air over Decomposing Substances, with special reference to their application to the Air of Sewers" (p. 28). Dr. Abraham Jacobi, of the College of Physicians and Surgeons of New York, then read a paper on "The Effect of Sewer Gas in the Production of Disease" (p. 62). Dr. Beverly Robinson, of the Bellevue Hospital Medical College, then read a paper on "Sewer Gas, a cause of throat disease (or the effect of bad drainage on the throat)" (p. 88). The time of the Climatological Association having expired, Dr. Vaughan, of Ann Arbor, and Dr. Welch, of Baltimore, who had been assigned to discuss this subject declined to speak.

THE AMERICAN DERMATOLOGICAL ASSOCIATION then assumed charge of the meeting and Dr. J. Nevins Hyde, of Rush Medical College of Chicago, read a paper on "The Distribution of Leprosy in North America" (p. 103). He was followed by Dr. Jas. E. Graham, of the Toronto University, on "The Distribution of Leprosy in the Dominion of Canada" (p. 117). Dr. P. A. Morrow, of the University of the City of New York, then read a paper on "The diagnostic features and treatment of Leprosy" (p. 121), which was discussed by Dr. Arthur Van Harlingen, of Philadelphia (p. 128). Dr. James C. White, of Harvard Medical School, then read a paper on "The Contagiousness and Control of Leprosy" (p. 130), which was followed by papers in discussion by Dr. Geo. Henry Fox, of the College of Physicians and Surgeons of New York (p. 138), and Dr. J. D. Bryant, of Bellevue Hospital Medical College (p. 139), and by Dr. Walter Wyman, Supervising Surgeon-General U. S. Marine Hospital Service, on the "National Control of Leprosy" (p. 145).

The third meeting of the Congress was held on Thursday, May 31st, it was opened under the direction of THE AMERICAN ASSOCIA-

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TION OF GENITO-URINARY SURGEONS, with a paper by Dr. Edward L. Keyes, of the City of New York, on "Nephritis in its Surgical Aspects" (p. 149), this was followed by a paper by Dr. George M. Sternberg, Surgeon-General United States Army, on "The Bacteriology of Pyelonephritis" (p. 171), and the subject was discussed by Dr. George Chismore of San Francisco, Cal. (p. 177), and Dr. F. S. Watson, of the Harvard Medical School (p. 178).

At 3.30 P. M., THE AMERICAN GYNECOLOGICAL SOCIETY, took charge of the meeting: Dr. Wm. M. Polk, of the University of the City of New York, read a paper on "The Conservative Surgery of the Female pelvic organs" (p. 182), followed by Dr. Wm. Goodell,* of the University of Pennsylvania, on "The Conservative treatment of the Female pelvic organs" (p. 195), and the subject was further discussed by Dr. Lutaud, of Paris, France (p. 202), Dr. M. D. Mann, of the University of Buffalo (p. 202), Dr. Joseph T. Johnson, of Washington, D. C. (p. 203), Dr. Howard A. Kelly, of Baltimore (p. 205), and Dr. Florian Krug, of New York City (p. 206).

The fourth meeting was held in the evening of Thursday, May 31st, to listen to THE ADDRESS OF THE PRESIDENT OF THE CONGRESS, Dr. Alfred L. Loomis, of the University of the City of New York. The meeting was presided over by Dr. George G. Harlan, President of the American Ophthalmological Society and ex-officio First Vice-President of the Congress. Dr. Loomis' address was on "The Influence of Animal Experimentation on Medical Science" (p. 299).

A meeting for business was held on June 1st, at 1.30 P. M., the President, Dr. A. L. Loomis in the chair.

Dr. J. Nevins Hyde, of Chicago, presented the following from the American Dermatological Association, and moved its adoption, viz:

WHEREAS, The American Dermatological Association recognizes the importance of the undoubted existence of Leprosy in North America: therefore

Resolved, That the Congress of American Physicians and Surgeons be requested to memorialize the Congress of the United States of America, with a view to the appointment of a commission of five to investigate the prevalence of Leprosy in the United States of America, and in countries upon its border: and to suggest means for the control of the disease.

* As these pages are going through the press the announcement is made of the death of the distinguished author of this paper, and these Transactions have the melancholy honor of containing his last message to his professional brethren. No one can read these eloquent words without being impressed by their lofty tone of thought, and by a sense of the dignity and conscientiousness in which he held his calling. The Congress and the medical profession have lost one of their most honorable and high-minded members. C.

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Dr. L. N. Duhring, of Philadelphia, seconded the motion; Dr. Graham, of Toronto, spoke in favor. The resolution was then adopted.

On motion it was directed that the resolution signed by the President and Secretary be transmitted to the proper committees of the Senate and House of Representatives.

Dr. L. McLane Tiffany, of Baltimore, moved the adoption of the following, viz:

WHEREAS, The Congress of American Physicians and Surgeons learns with great surprise and regret that the Army Appropriation bill which has recently passed the House, reduces the number of medical officers of the army by twenty-five, and also reduces the appropriation for the library of the Surgeon-General's Office from ten to seven thousand dollars;

Resolved, That it is the opinion of this Congress that these proposed reductions are unwise and are contrary to the best interests, not only of the Army, but of the medical profession of this country, and of all who depend upon them for skilled professional services, and that we respectfully request our Representatives and Senators to so amend this bill as to prevent the reductions above referred to.

Resolved, That copies of these resolutions duly certified by the President and Secretary of this Congress, be forwarded to the Speaker of the House of Representatives and to the presiding officer of the Senate with the request that they be presented and referred to the proper committees.

Adopted.

Dr. William H. Welch, of Johns Hopkins University, called attention to the fact that a bill had been presented in Congress forbidding experiments on animals in District of Columbia, and that there was a probability of the favorable report of the bill. He moved the adoption of the following, viz:

WHEREAS, The attempts in other countries to regulate by legislation the practice of experimentation upon animals have proven most disastrous to the progress of medical science and art;

Resolved, That the Congress of American Physicians and Surgeons enter their most earnest protest against any legislation tending to interfere with the advancement of medicine by means of experiments upon animals conducted by properly qualified persons.

Resolved, That a copy of these resolutions signed by the President and Secretary of this Congress be sent to the Speaker of the House and the presiding officer of the Senate, and to the proper committees of the Senate and House.

The resolutions were adopted.

The chairman of the Executive Committee called up the motion of Dr. White which had been referred from the Executive Committee to the Congress.

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This motion was to strike out Washington as the place of meeting and give the Executive Committee the power to determine the place of meeting.

Dr. Busey asked if this were not in the nature of an amendment to the by-laws and therefore have to lay over until the next meeting?

The President so decided.

Dr. Busey then moved:

That the consideration of Dr. White's motion be postponed and made the special order for the first day's session of the next Congress, and that it shall be the duty of the Secretary of the Executive Committee to call it up at that time.

Adopted.

The business meeting then adjourned.

The fifth general meeting of the Congress was held on Friday, June 1st, at 2 o'clock P. M., under the direction of THE AMERICAN LARYNGOLOGICAL ASSOCIATION. The subject was "The Surgery of the Accessory Sinuses of the Nose." Dr. F. H. Bosworth, of New York, read a paper on "Ethmoidal Disease" (p. 207). He was followed by Dr. J. H. Bryan, of Washington, D. C., on the "Surgery of the Maxillary Sinus" (p. 217), and by Dr. John N. Mackenzie, of Baltimore, on the "Surgery of the Accessory Sinuses" (p. 223).

At 3.30 P. M., THE AMERICAN NEUROLOGICAL ASSOCIATION assumed the direction of the Congress with a paper by Dr. James J. Putnam, of Boston, on the "Influence of Infectious Processes on the Nervous System" (p. 228). Dr. Charles K. Mills, of Philadelphia, then read a paper on the "Influence of Infectious Processes on Mental Disease" (p. 267), followed by Dr. F. X. Dercum, of Philadelphia, on the "Therapeutics of Infectious Diseases of the Nervous System" (p. 283).

The President then spoke as follows, viz:

Before closing the Congress there are a few words which I should like to say. I am informed that the registration at this Congress has been larger than at any previous one. I have also been informed as I have visited the various constituent societies, that attendance at the meetings has been larger than at any preceding Congress. There have been represented at the general sessions of the Congress seven different Associations, and the papers as I have listened to them have filled me with astonishment that we have reached such a high degree of culture in so many different departments of medicine.

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I would congratulate you upon the unity of feeling and action among the different societies in their support of the Congress, and it seems to me that they have become united more fully and more closely than ever before.

It is to be remembered that there have been new departures in this Congress. In the first place there has been a change in the time of meeting from the heat of September to the cool and pleasant weather of May. It seems to me that this change must commend itself, so far as convenience and comfort is concerned, to all of the members of the Congress. It has also established another fact, that Americans are able to carry on a Congress successfully alone. At the other Congresses gentlemen from abroad honored us with their presence and undoubtedly added much to the interest of our meetings. But the change in the time of the meeting of the Congress prevented them from being with us this year—it is almost impossible for those whom we would like to see from across the Atlantic, to leave home at this season of the year. Scores of letters have been received from our co-workers abroad regretting and deploring their inability to be with us on account of their work at home.

Gentlemen, I have to thank you for the courtesy you have extended to me during the whole of the work that has devolved upon me. I have been deeply interested in our success, for I was anxious to see if a Congress could be a success resting entirely upon an American basis.

Our dinner, I think, was also a success. At the first Congress the dinner was a failure and it was difficult to get your Executive Committee to consent to try another. It has been tried, and you can decide whether it has been a success. It has received the approval of all with whom I have spoken.

This Congress is one of the most important medical bodies in the country, for it brings together skilled workers in all departments of medicine and surgery. It brings forth unity of thought and action among medical men. It brings us in close touch with each other, so that we receive inspiration for better work; and if there is one body more than another in this country that is to raise our profession from the lower grade which it has so long occupied, it seems to me that it is just such a body as has been assembling here for the past four days.

With many good wishes, gentlemen, I bid you good-bye.

Adjourned.

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GUESTS OF THE CONGRESS,

By invitation of the Executive Committee.

Gen. George M. Sternberg, Surgeon General U. S. A., Washington, D.C.
Dr. Lutaud, Paris, France.
Dr. Wm. Pepper, Pres. of the Pan-American Medical Congress, Philadelphia, Pa.
Dr. Wm. P. McGuire, Pres. the Medical Society of Virginia, Winchester, Va.

MEMBERS OF THE CONGRESS.

Absent members are indicated by *.

AMERICAN OPHTHALMOLOGICAL SOCIETY.

Organized June, 1864.

ACTIVE MEMBERS.

*Dr. F. W. Abbott,	Buffalo, New York.
*Dr. Harlan P. Allen,	Columbus, Ohio.
*Dr. A. Alt,	St. Louis, Missouri.
*Dr. J. A. Andrews,	New York City.
*Dr. S. C. Ayres,	Cincinnati, Ohio.
*Dr. William T. Bacon,	Hartford, Connecticut.
*Dr. B. J. Baldwin,	Montgomery, Alabama.
*Dr. Edwin W. Bartlett,	Milwaukee, Wisconsin.
Dr. David DeBeck,	Cincinnati, Ohio.
*Dr. H. W. Bradford,	Boston, Massachusetts.
Dr. Charles S. Bull,	New York City.
*Dr. F. Buller,	Montreal, Canada.
*Dr. Swan M. Burnett,	Washington, D. C.
*Dr. Peter A. Callan,	New York City.
*Dr. F. P. Capron,	Providence, Rhode Island.
Dr. W. H. Carmalt,	New Haven, Connecticut.
*Dr. H. B. Chandler,	Boston, Massachusetts.
*Dr. William Cheatham,	Louisville, Kentucky.
*Dr. F. S. Cheney,	Boston, Massachusetts.
Dr. C. F. Clark,	Columbus, Ohio.
*Dr. David Coggin,	Salem, Massachusets.
Dr. F. S. Coolidge,	Chicago, Illinois.
Dr. C. M. Culver,	Albany, New York.
*Dr. W. S. Dennett,	New York City.
*Dr. Hasket Derby,	Boston, Massachusetts.
*Dr. Richard H. Derby,	New York City.

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*Dr. Lewis S. Dixon,	Boston, Massachusetts.
Dr. J. B. Emerson,	New York City.
*Dr. Arthur E. Ewing,	St. Louis, Missouri.
*Dr. George F. Fisk,	Chicago, Illinois.
*Dr. Edward Friedenburg,	New York City.
Dr. B. E. Fryer,	Kansas City, Missouri.
Dr. John Green,	St. Louis, Missouri.
*Dr. Emil Gruening,	New York City.
*Dr. G. W. Hale,	Nashville, Tennessee.
*Dr. A. D. Hall,	Philadelphia, Pennsylvania.
Dr. H. F. Hansell,	Philadelphia, Pennsylvania.
Dr. George C. Harlan,	Philadelphia, Pennsylvania.
*Dr. David Harrower,	Worcester, Massachusetts.
Dr. Gustavus Hay,	Boston, Massachusetts.
Dr. Niel J. Hepburn,	New York City.
Dr. Albert G. Heyl,	Philadelphia, Pennsylvania.
*Dr. Edward L. Holmes,	Chicago, Illinois.
*Dr. E. E. Holt,	Portland, Maine.
Dr. Lucien Howe,	Buffalo, New York.
*Dr. D. W. Hunter,	New York City.
*Dr. Edward Jackson,	Philadelphia, Pennsylvania.
*Dr. B. Joy Jeffries,	Boston, Massachusetts.
Dr. W. B. Johnson,	Paterson, New Jersey.
*Dr. Samuel J. Jones,	Chicago, Illinois.
*Dr. Charles J. Kipp,	Newark, New Jersey.
Dr. H. Knapp,	New York City.
*Dr. Carl Koller,	New York City.
Dr. Charles W. Kollock,	Charleston, South Carolina.
*Dr. W. E. Lambert,	New York City.
*Dr. Richmond Lennox,	Brooklyn, New York.
Dr. J. A. Lippincott,	Pittsburgh, Pennsylvania.
Dr. F. B. Loring,	Washington, D. C.
*Dr. F. W. Marlow,	Syracuse, New York.
Dr. W. V. Marmion,	Washington, D. C.
*Dr. Arthur Mathewson,	Brooklyn, New York.
*Dr. S. F. McFarland,	Binghamton, New York.
*Dr. Charles H. McIlwaine,	Trenton, New Jersey.
*Dr. R. J. McKay,	Wilmington, Delaware.
*Dr. C. S. Merrill,	Albany, New York.
Dr. B. L. Milliken,	Cleveland, Ohio.
*Dr. H. G. Miller,	Providence, Rhode Island.
*Dr. J. Lancelot Minor,	Memphis, Tennessee.
*Dr. W. F. Mittendorf,	New York City.
*Dr. William O. Moore,	New York City.
*Dr. Russell Murdock,	Baltimore, Maryland.
*Dr. William F. Norris,	Philadelphia, Pennsylvania.
*Dr. Henry D. Noyes,	New York City.
*Dr. J. F. Noyes,	Detroit, Michigan.
Dr. Charles A. Oliver,	Philadelphia, Pennsylvania.

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Dr. H. S. Oppenheimer,	New York City.
*Dr. O. D. Pomeroy,	New York City.
*Dr. Thomas R. Pooley,	New York City.
*Dr. M. H. Post,	St. Louis, Missouri.
*Dr. J. S. Prout,	Brooklyn, New York.
Dr. B. Alexander Randall,	Philadelphia, Pennsylvania.
*Dr. R. L. Randolph,	Baltimore, Maryland.
*Dr. William Rankin,	Newark, New Jersey.
*Dr. J. M. Ray,	Louisville, Kentucky.
Dr. R. A. Reeve,	Toronto, Canada.
Dr. Stephen O. Richey,	Washington, D. C.
*Dr. Charles E. Rider,	Rochester, New York.
*Dr. Wheelock Rider,	Rochester, New York.
*Dr. Frank W. Ring,	New York City.
Dr. Samuel D. Risley,	Philadelphia, Pennsylvania.
*Dr. Edmund C. Rivers,	Denver City, Colorado.
*Dr. D. B. St. John Roosa,	New York City.
Dr. John D. Rushmore,	Brooklyn, New York.
*Dr. Robert Sattler,	Cincinnati, Ohio.
Dr. G. E. de Schweinitz,	Philadelphia, Pennsylvania.
*Dr. W. W. Seeley,	Cincinnati, Ohio.
*Dr. Henry L. Shaw,	Boston, Massachusetts.
*Dr. J. A. Spalding,	Portland, Maine.
*Dr. Francis P. Sprague,	Boston, Massachusetts.
*Dr. Myles Standish,	Boston, Massachusetts.
*Dr. George T. Stevens,	New York City.
Dr. S. B. St. John,	Hartford, Connecticut.
*Dr. George Strawbridge,	Philadelphia, Pennsylvania.
*Dr. T. Y. Sutphen,	Newark, New Jersey.
*Dr. J. Oscroft Tansley,	New York City.
*Dr. Lewis H. Taylor,	Wilkesbarre, Pennsylvania.
*Dr. Samuel Theobald,	Baltimore, Maryland.
Dr. Chas. Hermon Thomas,	Philadelphia, Pennsylvania.
*Dr. William Thomson,	Philadelphia, Pennsylvania.
*Dr. John VanDuyn,	Syracuse, New York.
Dr. J. J. B. Vermyne,	New Bedford, Massachusetts.
Dr. O. F. Wadsworth,	Boston, Massachusetts.
*Dr. Le Roy P. Walker,	New York City.
*Dr. David Webster,	New York City.
*Dr. J. E. Weeks,	New York City.
Dr. J. A. White,	Richmond, Virginia.
*Dr. Charles H. Williams,	Chicago, Illinois.
*Dr. Henry W. Williams,	Boston, Massachusetts.
Dr. F. M. Wilson,	Bridgeport, Connecticut.
*Dr. J. P. Worrell,	Terre Haute, Indiana.

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HONORARY MEMBER.

*Dr. C. Schweigger,	Berlin, Germany.

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AMERICAN OTOLOGICAL SOCIETY.

Organized, July, 1866.

ACTIVE MEMBERS.

*Dr. F. W. Abbott,	Buffalo, New York.
*Dr. John L. Adams,	New York City.
*Dr. H. P. Allen,	Columbus, Ohio.
*Dr. A. Alt,	St. Louis, Missouri.
*Dr. J. A. Andrews,	New York City.
*Dr. S. C. Ayres,	Cincinnati, Ohio.
Dr. Gorham Bacon,	New York City.
*Dr. B. J. Baldwin,	Montgomery, Alabama.
*Dr. Robert Barclay,	St. Louis, Missouri.
*Dr. E. W. Bartlett,	Milwaukee, Wisconsin.
Dr. C. J. Blake,	Boston, Massachusetts.
*Dr. R. O. Born,	New York City.
*Dr. A. H. Buck,	New York City.
*Dr. C. H. Burnett,	Philadelphia, Pennsylvania.
*Dr. F. P. Capron,	Providence, Rhode Island.
Dr. W. H. Carmalt,	New Haven, Connecticut.
*Dr. J. Elliott Colburn,	Chicago, Illinois.
*Dr. William Cowen,	New York City.
Dr. Edward B. Dench,	New York City.
Dr. J. B. Emerson,	New York City.
*Dr. Edward Friedenburg,	New York City.
Dr. B. E. Fryer, U. S. A.	Kansas City, Missouri.
Dr. John Green,	St. Louis, Missouri.
*Dr. J. Orne Green,	Boston, Massachusetts.
*Dr. Emil Gruening,	New York City.
*Dr. G. W. Hale,	Nashville, Tennessee.
Dr. G. C. Harlan,	Philadelphia, Pennsylvania.
*Dr. David Harrower, Jr.,	Worcester, Massachusetts.
Dr. N. J. Hepburn,	New York City.
*Dr. E. L. Holmes,	Chicago, Illinois.
*Dr. E. E. Holt,	Portland, Maine.
*Dr. W. H. Hotchkiss,	St. Louis, Missouri.
Dr. Lucien Howe,	Buffalo, New York.
Dr. F. L. Jack,	Boston, Massachusetts.
Dr. W. B. Johnson,	Paterson, New Jersey.
*Dr. S. J. Jones,	Chicago, Illinois.
*Dr. Charles J. Kipp,	Newark, New Jersey.
Dr. Hermann Knapp,	New York City.
Dr. J. A. Lippincott,	Pittsburgh, Pennsylvania.
Dr. F. B. Loring,	Washington, D. C.
*Dr. Arthur Mathewson,	Brooklyn, New York.
*Dr. R. J. McKay,	Wilmington, Delaware.
*Dr. C. S. Merrill,	Albany, New York.

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*Dr. H. G. Miller,	Providence, Rhode Island.
Dr. B. L. Milliken,	Cleveland, Ohio.
*Dr. H. L. Morse,	Boston, Massachusetts.
Dr. Robert C. Myles,	New York City.
*Dr. H. D. Noyes,	New York City.
*Dr. J. F. Noyes,	Detroit, Michigan.
*Dr. O. D. Pomeroy,	New York City.
*Dr. T. R. Pooley,	New York City.
*Dr. J. S. Prout,	Brooklyn, New York.
Dr. B. Alexander Randall,	Philadelphia, Pennsylvania.
*Dr. William Rankin, Jr.,	Newark, New Jersey.
*Dr. J. M. Ray,	Louisville, Kentucky.
Dr. R. A. Reeve,	Toronto, Canada.
*Dr. Huntington Richards,	Des Moines, Iowa.
Dr. S. O. Richey,	Washington, D. C.
*Dr. Charles E. Rider,	Rochester, New York.
*Dr. Wheelock Rider,	Rochester, New York.
*Dr. F. W. Ring,	New York City.
Dr. S. D. Risley,	Philadelphia, Pennsylvania.
*Dr. D. B. St. John Roosa,	New York City.
Dr. J. D. Rushmore,	Brooklyn, New York.
*Dr. W. W. Seely,	Cincinnati, Ohio.
*Dr. R. W. Seiss,	Philadelphia, Pennsylvania.
*Dr. Samuel Sexton,	New York City.
*Dr. J. B. Shapleigh,	St. Louis, Missouri.
*Dr. H. L. Shaw,	Boston, Massachusetts.
*Dr. E. D. Spear,	Boston, Massachusetts.
*Dr. H. N. Spencer,	St. Louis, Missouri.
*Dr. G. T. Stevens,	New York City.
Dr. S. B. St. John,	Hartford, Connecticut.
*Dr. George Strawbridge,	Philadelphia, Pennsylvania.
*Dr. T. Y. Sutphen,	Newark, New Jersey.
*Dr. J. O. Tansley,	New York City.
Dr. Samuel Theobald,	Baltimore, Maryland.
Dr. William Thomson,	Philadelphia, Pennsylvania.
Dr. Robert Tilley,	Chicago, Illinois.
*Dr. C. A. Todd,	St. Louis, Missouri.
*Dr. John Van Duyn,	Syracuse, New York.
Dr. J. J. B. Vermyne,	New Bedford, Massachusetts.
*Dr. David Webster,	New York City.
Dr. Wm. H. Wilmer,	Washington, D. C.
Dr. F. M. Wilson,	Bridgeport, Connecticut.

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HONORARY MEMBERS.

*Prof. A. Graham Bell,	Washington, D. C.
*Prof. A. M. Mayer,	Hoboken, New Jersey.
*Prof. O. N. Rood,	New York City.

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AMERICAN NEUROLOGICAL ASSOCIATION.

Organized, June, 1875.

ACTIVE MEMBERS.

*Dr. E. B. Angell,	Rochester, New York.
Dr. Samuel Ayers,	Pittsburgh, Pennsylvania.
*Dr. L. W. Baker,	Baldwinsville, Massachusetts.
*Dr. Smith Baker,	Utica, New York.
*Dr. H. M. Bannister,	Kankakee, Illinois.
*Dr. Roberts Bartholow,	Philadelphia, Pennsylvania.
*Dr. W. R. Birdsall,	New York City.
*Dr. J. A. Booth,	New York City.
*Dr. N. E. Brill,	New York City.
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*Dr. Samuel Pozzi,	Paris, France.
*Dr. W. O. Priestly,	London, England.
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*Dr. J. Wright,	Brooklyn, New York.

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Dr. N. P. Dandridge,	Cincinnati, Ohio.
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Dr. W. M. Mastin,	Mobile, Alabama.
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Dr. S. J. Mixter,	Boston, Massachusetts.
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Dr. John Parmenter,	Buffalo, New York.
Dr. L. S. Pilcher,	Brooklyn, New York.
Dr. C. B. Porter,	Boston, Massachusetts.
*Dr. W. G. Porter,	Philadelphia, Pennsylvania.
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*Dr. Joseph Ransohoff,	Cincinnati, Ohio.
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*Dr. Stephen Smith,	New York City.
*Dr. L. A. Stimson,	New York City.
*Dr. W. E. Taylor,	San Francisco, California.
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Dr. William Thomson,	Philadelphia, Pennsylvania.
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Dr. J. R. Weist,	Richmond, Indiana.
Dr. H. R. Wharton,	Philadelphia, Pennsylvania.
*Dr. J. William White,	Philadelphia, Pennsylvania.
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*Dr. John Cheine, F. R. C. S.,	Edinburgh, Scotland.
*Prof. Dr. Vinzenz Czerny,	Heidelberg, Germany.
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*Dr. J. E. Erichson, F. R. C. S.,	London, England.
*Prof. Dr. F. v. Esmarch,	Kiel, Germany.
*Dr. Carl Gussenbauer,	Vienna, Germany.
*Dr. Reginald Harrison, F. R. C. S.,	London, England.
*Dr. Victor Horsley, F. R. C. S.,	London, England.
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*Dr. William Hunt, M. D.,	Philadelphia, Pennsylvania.
*Dr. Theodore Kocker,	Berne, Switzerland.
*Sir Joseph Lister,	London, England.
*Sir William MacCormac, Bart., F.R.C.S.I., F.R.C.S.,	London, England.
*Dr. William MacEwen,	Glasgow, Scotland.
*Prof. Dr. Leopold Ollier,	Lyons, France.
*Sir James Paget, Bart., F.R.C.S.,	London, England.
*Dr. Jules E. Pean,	Paris, France.
*Dr. Max Schede,	Hamburg, Germany.
*Dr. Carl Thiersch,	Leipsic, Germany.
*Prof. A. Verneuil,	Paris, France.
*Sir T. Spencer Wells, Bart.	London, England.

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Organized September, 1883.

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*Dr. H. B. Baker,	Lansing, Michigan.
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Dr. F. H. Bosworth,	New York City.
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*Dr. D. M. Camman,	New York City.
*Dr. S. H. Chapman,	New Haven, Connecticut.
*Dr. T. M. Coan,	New York City.
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*Dr. T. D. Coleman,	Augusta, Georgia.
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Dr. R. G. Curtin,	Philadelphia, Pennsylvania.
Dr. Judson Daland,	Philadelphia, Pennsylvania.
Dr. W. H. Daly,	Pittsburgh, Pennsylvania.
*Dr. Thomas Darlington,	Bisbee, Arizona.
*Dr. Charles Denison,	Denver, Colorado.
*Dr. H. D. Didama,	Syracuse, New York.
*Dr. H. O. Dodge,	Boulder, Colorado.
*Dr. C. P. Donaldson,	Muskegon, Michigan.
*Dr. William F. Edwards,	San Diego, California.
Dr. H. L. Elsner,	Syracuse, New York.
*Dr. J. T. Eskridge,	Denver, Colorado.
Dr. S. A. Fisk,	Denver, Colorado.
Dr. W. E. Ford,	Utica, New York.
Dr. T. R. French,	Brooklyn, New York.
*Dr. G. M. Garland,	Boston, Massachusetts.
*Dr. A. S. Garnett,	Hot Springs, Arkansas.
Dr. William M. Gibson,	Utica, New York.
Dr. A. L. Gihon, U. S. N.,	Brooklyn, New York.
Dr. W. C. Glasgow,	St. Louis, Missouri.
*Dr. Landon Carter Gray,	New York City.
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Dr. A. Jacobi,	New York City.
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*Dr. J. S. Orme,	Los Angeles, California.
Dr. E. O. Otis,	Boston, Massachusetts.
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HONORARY MEMBERS.

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*Dr. Alfred Stillé,	Philadelphia, Pennsylvania.

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ASSOCIATION OF AMERICAN PHYSICIANS.

Organized October, 1885.

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Dr. H. M. Hurd,	Baltimore, Maryland.
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Dr. E. G. Janeway,	New York City.
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*Dr. M. J. Lewis,	Philadelphia, Pennsylvania.
Dr. A. L. Loomis,	New York City.
*Dr. Starling Loving,	Columbus, Ohio.
Dr. W. T. Lusk,	New York City.
Dr. H. M. Lyman,	Chicago, Illinois.
*Dr. A. McPhedran,	Toronto, Canada.
*Dr. A. L. Mason,	Boston, Massachusetts.
*Dr. A. V. Meigs,	Philadelphia, Pennsylvania.
Dr. S. J. Meltzer,	New York City.
*Dr. F. T. Miles,	Baltimore, Maryland.
*Dr. S. Weir Mitchell,	Philadelphia, Pennsylvania.
Dr. J. H. Musser,	Philadelphia, Pennsylvania.
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Dr. William Osler,	Baltimore, Maryland.
Dr. G. L. Peabody,	New York City.
*Dr. William Pepper,	Philadelphia, Pennsylvania.
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Dr. D. W. Prentiss,	Washington, D. C.
Dr. T. Mitchell Prudden,	New York City.
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Dr. J. J. Putnam,	Boston, Massachusetts.
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Dr. G. B. Shattuck,	Boston, Massachusetts.
Dr. F. C. Shattuck,	Boston, Massachusetts.
Dr. Wharton Sinkler,	Philadelphia, Pennsylvania.
Dr. A. A. Smith,	New York City.
Dr. A. H. Smith,	New York City.
Dr. Theobald Smith,	Washington, D. C.
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Dr. Louis Starr,	Philadelphia, Pennsylvania.
Dr. M. Allen Starr,	New York City.
*Dr. G. M. Sternberg, U. S. A.,	Baltimore, Maryland.
Dr. James Stewart,	Montreal, Canada.
Dr. C. G. Stockton,	Buffalo, New York.
Dr. W. Gilman Thompson,	New York City.
Dr. W. H. Thomson,	New York City.
*Dr. E. L. Trudeau,	Saranac Lake, New York.
Dr. James Tyson,	Philadelphia, Pennsylvania.
Dr. V. C. Vaughan,	Ann Arbor, Michigan.
Dr. H. P. Walcott,	Cambridge, Massachusetts.
*Dr. S. B. Ward,	Albany, New York.
Dr. W. H. Welch,	Baltimore, Maryland.
*Dr. J. T. Whittaker,	Cincinnati, Ohio.
Dr. George Wilkins,	Montreal, Canada.
Dr. J. C. Wilson,	Philadelphia, Pennsylvania.
Dr. H. C. Wood,	Philadelphia, Pennsylvania.

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HONORARY MEMBERS.

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*Dr. J. T. Metcalf,	New York City.
*Dr. Francis Minot,	Boston, Massachusetts.
*Dr. Alfred Stillé,	Philadelphia, Pennsylvania.

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AMERICAN ASSOCIATION OF GENITO-URINARY SURGEONS.

Organized October, 1886.

ACTIVE MEMBERS.

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Dr. G. W. Allen,	Boston, Massachusetts.
Dr. L. Bolton Bangs,	New York City.
Dr. W. T. Belfield,	Chicago, Illinois.
Dr. James Bell,	Montreal, Canada.
*Dr. Charles F. Bevan,	Baltimore, Maryland.
*Dr. G. E. Brewer,	New York City.
Dr. F. Tilden Brown,	New York City.
Dr. J. P. Bryson,	St. Louis, Missouri.

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Dr. E. C. Burnett,	St. Louis, Missouri.
*Dr. A. T. Cabot,	Boston, Massachusetts.
*Dr. N. B. Carson,	St. Louis, Missouri.
Dr. George Chismore,	San Francisco, California.
*Dr. E. M. Culver,	New York City.
Dr. J. A. Fordyce,	New York City.
Dr. G. R. Fowler,	Brooklyn, New York.
*Dr. A. S. Garnett,	Hot Springs, Arkansas.
*Dr. W. F. Glenn,	Nashville, Tennessee.
*Dr. F. B. Greenough,	Boston, Massachusetts.
Dr. J. R. Hayden,	New York City.
Dr. W. H. Hingston,	Montreal, Canada.
Dr. J. Nevins Hyde,	Chicago, Illinois.
Dr. William Judkins,	Cincinnati, Ohio.
Dr. E. L. Keyes,	New York City.
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Dr. Bransford Lewis,	St. Louis, Missouri.
*Dr. Charles McBurney,	New York City.
Dr. Edward Martin,	Philadelphia, Pennsylvania.
Dr. C. H. Mastin,	Mobile, Alabama.
Dr. W. M. Mastin,	Mobile, Alabama.
*Dr. P. A. Morrow,	New York City.
*Dr. H. G. Mudd,	St. Louis, Missouri.
*Dr. F. N. Otis,	New York City.
Dr. W. K. Otis,	New York City.
*Dr. E. R. Palmer,	Louisville, Kentucky.
Dr. Roswell Park,	Buffalo, New York.
Dr. Abner Post,	Boston, Massachusetts.
*Dr. C. L. Scudder,	Boston, Massachusetts.
Dr. A. W. Stein,	New York City.
*Dr. F. R. Sturgis,	New York City.
Dr. R. W. Taylor,	New York City.
*Dr. G. H. Tilden,	Boston, Massachusetts.
*Dr. Paul Thorndike,	Boston, Massachusetts.
*Dr. J. P. Tuttle,	New York City.
*Dr. F. S. Watson,	Boston, Massachusetts.
Dr. J. Blake White,	New York City.
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Dr. W. M. Wishard,	Indianapolis, Indiana.

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AMERICAN ORTHOPŒDIC ASSOCIATION.

Organized May, 1887.

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Dr. Wallace Blanchard,	Chicago, Illinois.
Dr. E. H. Bradford,	Boston, Massachusetts.

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Dr. Dillon Brown,	New York City.
Dr. Joseph D. Bryant,	New York City.
Dr. Herbert L. Burrell,	Boston, Massachusetts.
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Dr. Ansel G. Cook,	Hartford, Connecticut.
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Dr. V. P. Gibney,	New York City.
Dr. Arthur J. Gillette,	St. Paul, Minnesota.
*Dr. H. E. Goodman,	Philadelphia, Pennsylvania.
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*Dr. J. D. Griffith,	Kansas City, Nebraska.
*Dr. A. E. Hoadley,	Chicago, Illinois.
*Dr. Harry Hodgen,	St. Louis, Missouri.
Dr. A. B. Hosmer,	Chicago, Illinois.
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*Dr. A. B. Judson,	New York City.
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Dr. Benjamin Lee,	Philadelphia, Pennsylvania.
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Dr. S. L. McCurdy,	Dennison, Ohio.
*Dr. B. E. McKenzie,	Toronto, Canada.
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*Dr. George B. Packard,	Denver, Colorado.
Dr. Roswell Park,	Buffalo, New York.
Dr. A. M. Phelps,	New York City.
*Dr. John Ridlon,	Chicago, Illinois.
*Dr. W. O. Plympton,	New York City.
Dr. George W. Ryan,	Cincinnati, Ohio.
*Dr. Lewis A. Sayre,	New York City.
*Dr. Reginald H. Sayre,	New York City.
*Dr. John C Schapps,	Brooklyn, New York.
*Dr. Charles Locke Scudder,	Boston, Massachusetts.
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*Dr. Harry M. Sherman,	San Francisco, California.
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Dr. Henry Ling Taylor,	New York City.
Dr. Wm. J. Taylor,	Philadelphia, Pennsylvania.
*Dr. Augustus Thorndike,	Boston, Massachusetts.
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*Dr. Henry G. Davis,	Everett, Massachusetts.
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*Dr. W. J. Little,	London, England.
*Dr. William MacEwen,	Glasgow, Scotland.
*Dr. Leopold Ollier,	Lyons, France.
*Dr. C. Fayette Taylor,	New York City.

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*Dr. Robert Jones,	Liverpool, England.
*Dr. E. Kermisson,	Paris, France.
*Dr. Siegfred Levy,	Copenhagen, Denmark.
*Dr. E. Muirhead Little,	London, England.
*Dr. Adolf Lorenz,	Vienna, Austria.
*Dr. Howard Marsh,	London, England.
*Dr. Edmund Owen,	London, England.
*Dr. Pietro Panzeri,	Milan, Italy.
*Dr. Robert Wm. Parker,	London, England.
*Dr. Paul Redard,	Paris, France.
*Dr. Bernard Roth,	London, England.
*Dr. Noble Smith,	London, England.
*Dr. W. J. Walsham,	London, England.
*Dr. George Arthur Wright,	Manchester, England.
*Dr. Julius Dollinger,	Buda-Pesth, Austria.

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AMERICAN PHYSIOLOGICAL SOCIETY.

Organized December, 1887.

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Dr. A. P. Brubaker,	Philadelphia, Pennsylvania.
*Dr. J. P. Campbell,	Athens, Georgia.
*Dr. H. C. Chapman,	Philadelphia, Pennsylvania.
*Dr. R. H. Chittenden,	New Haven, Connecticut.
*Dr. W. T. Councilman,	Baltimore, Maryland.
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Dr. H. H. Donaldson,	Worcester, Massachusetts.

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*Dr. C. F. Hodge,	Madison, Wisconsin.
Dr. W. H. Howell,	Baltimore, Maryland.
Dr. G. T. Kemp,	Brooklyn, New York.
Dr. F. S. Lee,	Bryn Mawr, Pennsylvania.
*Dr. Jacques Loeb,	Chicago, Illinois.
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Dr. Graham Lusk,	New Haven, Connecticut.
Dr. F. Mall,	Worcester, Massachusetts.
Dr. John Marshall,	Philadelphia, Pennsylvania.
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*Dr. S. J. Meltzer,	New York City.
*Dr. T. W. Mills,	Montreal, Canada.
*Dr. C. S. Minot,	Boston, Massachusetts.
*Dr. S. Weir Mitchell,	Philadelphia, Pennsylvania.
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Dr. William Osler,	Baltimore, Maryland.
Dr. Isaac Ott,	Easton, Pennsylvania.
Dr. Wm. T. Porter,	Boston, Massachusetts.
*Dr. E. T. Reichert,	Philadelphia, Pennsylvania.
*Dr. W. T. Sedgwick,	Boston, Massachusetts.
*Dr. Henry Sewall,	Denver, Colorado.
Dr. H. E. Smith,	New Haven, Connecticut.
*Dr. R. Meade Smith,	Philadelphia, Pennsylvania.
*Dr. G. M. Sternberg, U. S. A.,	Washington, D. C.
Dr. G. N. Stewart,	Boston, Massachusetts.
Dr. W. G. Thompson,	New York City.
Dr. V. C. Vaughan,	Ann Arbor, Michigan.
Dr J. W. Warren,	Boston, Massachusetts.
Dr. William H. Welch,	Baltimore, Maryland.
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Dr. H. C. Wood,	Philadelphia, Pennsylvania.

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ASSOCIATION OF AMERICAN ANATOMISTS.

Organized September 17th, 1888.

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*Dr. Charles C. Barrell,	Galveston, Texas.
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*Dr. C. B. Ewing, U. S. A.	St. Louis, Missouri.
*Prof. P. A. Fish,	Ithaca, New York.
Dr. W. S. Forbes,	Philadelphia, Pennsylvania.
*Dr. C. L. Ford,	Ann Arbor, Michigan.
*Dr. J. P. C. Foster,	New Haven, Connecticut.
Dr. Frank R. Fry,	St. Louis, Missouri.
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*Dr. S. J. J. Harger,	Philadelphia, Pennsylvania.
*Dr. Jas. Wm. Hartigan,	Morgantown, West Virginia.
*Dr. I. S. Haynes,	New York City.
Dr. Charles Heitzmann,	New York City.
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*Dr. E. W. Holmes,	Philadelphia, Pennsylvania.
*Dr. R. S. Huidekoper,	Philadelphia, Pennsylvania.
*Dr. Geo. S. Huntington,	New York City.
*Dr. Horace Jayne,	Philadelphia, Pennsylvania.
Dr. G. T. Kemp,	Brooklyn, New York.
Dr. D. S. Lamb,	Washington, D. C.
*Dr. F. W. Langdon,	Cincinnati, Ohio.
*Dr. Thomas G. Lee,	Cambridge, Massachusetts.
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Mr. F. A. Lucas,	Washington, D. C.

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*Dr. R. Matas,	New Orleans, Louisiana.
*Dr. Washington Matthews, U.S.A.,	Ft. Wingate, New Mexico.
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*Dr. Middleton Michel,	Charleston, South Carolina.
*Dr. J. S. Miller,	Philadelphia, Pennsylvania.
*Prof. Chas. S. Minot,	Boston, Massachusetts.
Dr. S. J. Mixter,	Boston, Massachusetts.
Dr. R. O. Moody,	New Haven, Connecticut.
Dr. M. B. Moody,	New Haven, Connecticut.
Dr. J. F. Moran,	Washington, D. C.
Dr. J. D. Morgan,	Washington, D. C.
*Dr. J. C. Munro,	Boston, Massachusetts.
*Dr. W. E. Norris,	Lincoln, Nebraska.
*Dr. H. F. Osborn,	Princeton, New Jersey.
*Dr. C. M. E. Parkhill,	Denver, Colorado.
Dr. John Parmenter,	Buffalo, New York.
*Dr. G. A. Piersol,	Philadelphia, Pennsylvania.
*Dr. Robert Reyburn,	Washington, D. C.
Dr. M. H. Richardson,	Boston, Massachusetts.
*Dr. J. B. Roberts,	Philadelphia, Pennsylvania.
*Dr. J. A. Ryder,	Philadelphia, Pennsylvania.
*Dr. F. C. Schaefer,	Chicago, Illinois.
*Prof. Wm. B. Scott,	Princeton, New Jersey.
*Dr. F. J. Shepherd,	Montreal, Canada.
*Dr. R. W. Shufeldt,	Washington, D. C.
*Dr. D. K. Shute,	Washington, D. C.
Dr. E. Souchon,	New Orleans, Louisiana.
*Dr. E. C. Spitzka,	New York City.
*Dr. T. B. Stowell,	Potsdam, New York.
*Dr. W. X. Sudduth,	Minneapolis, Minnesota.
*Dr. H. E. Summers,	Knoxville, Tennessee.
*Dr. W. B. Towles,	Charlottesville, Virginia.
*Dr. A. H. Tuttle,	Charlottesville, Virginia.
*Dr. F. D. Weisse,	New York City.
Dr. G. W. West,	Washington, D. C.
Dr. B. G. Wilder,	Ithaca, New York.
*Dr. S. W. Williston,	Lawrence, Kansas.
*Dr. W. L. Wills,	Los Angeles, California.
*Dr. W. P. Wilson,	Philadelphia, Pennsylvania.
*Dr. J. L. Wortman,	Washington, D. C.
*Dr. R. R. Wright,	Toronto, Canada.

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HONORARY MEMBERS.

*Mr. William Anderson, F.R.C.S.	London, England.
*Dr. D. J. Cunningham,	Dublin, Ireland.

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AMERICAN PEDIATRIC SOCIETY.

Organized September 18th, 1888.

ACTIVE MEMBERS.

*Dr. George N. Acker,	Washington, D. C.
*Dr. S. S. Adams,	Washington, D. C.
*Dr. A. D. Blackader,	Montreal, Canada.
Dr. W. D. Booker,	Baltimore, Maryland.
Dr. Dillon Brown,	New York City.
*Dr. E. Buckingham,	Boston, Massachusetts.
Dr. S. C. Busey,	Washington, D. C.
Dr. A. Caillé,	New York City.
*Dr. W. L. Carr,	New York City.
*Dr. H. D. Chapin,	New York City.
Dr. F. M. Crandall,	New York City.
Dr. W. S. Christopher,	Chicago, Illinois.
*Dr. C. W. Earle,	Chicago, Illinois.
Dr. F. Forchheimer,	Cincinnati, Ohio.
Dr. J. H. Fruitnight,	New York City.
*Dr. J. P. Crozer-Griffith,	Philadelphia, Pennsylvania.
*Dr. M. P. Hatfield,	Chicago, Illinois.
Dr. L. Emmett Holt,	New York City.
*Dr. F. Huber,	New York City.
*Dr. H. Jackson,	Boston, Massachusetts.
Dr. A. Jacobi,	New York City.
*Dr. J. M. Keating,†	Colorado Springs, Colorado.
Dr. Henry Koplik,	New York City.
*Dr. H. Lafleur,	Montreal, Canada.
*Dr. T. S. Latimer,	Baltimore, Maryland.
*Dr. William F. Lockwood,	Baltimore, Maryland.
*Dr. F. Gordon Morrill,	Boston, Massachusetts.
Dr. W. P. Northrup,	New York City.
*Dr. J. O'Dwyer,	New York City.
Dr. William Osler,	Baltimore, Maryland.
*Dr C. P. Putnam,	Boston, Massachusetts.
Dr B. K. Rachford,	Cincinnati, Ohio.
Dr. T. M. Rotch,	Boston, Massachusetts.
Dr. B. Scharlan,	New York City.
Dr. A. Seibert,	New York City.
*Dr. T. F. Sherman,	Boston, Massachusetts.
*Dr. J. Lewis Smith,	New York City.
Dr. I. M. Snow,	Buffalo, New York.
Dr. Louis Starr,	New York City.
*Dr. C. W. Townsend,	Boston, Massachusetts.
Dr. V. C. Vaughan,	Ann Arbor, Michigan.
Dr. W. P. Watson,	Jersey City, New Jersey.
D . J. C. Wilson,	Philadelphia, Pennsylvania.
Dr. J. E. Winters,	New York City.
*Dr. L. M. Yale,	New York City.

† Deceased.

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The following gentlemen were present as

VISITORS

to various of the component Associations.

Dr. Adolph Behrend,	Washington, D. C.
Dr. Charles Beer,	Batavia, Ohio.
Dr. I. Berman,	Washington, D. C.
Dr. Frank Billings,	Chicago, Illinois.
Dr. R. W. Bishop,	Chicago, Illinois.
Dr. L. Bremer,	St. Louis, Missouri.
Dr. J. R. Bromwell,	Washington, D. C.
Dr. F. W. Chapin,	Hot Springs, Virginia.
Dr. H. R. Chears,	Philadelphia, Pennsylvania.
Dr. Carter S. Cole,	New York City.
Dr. T. W. Corwin,	Newark, New Jersey.
Dr. S. Cummings,	Hamilton, Canada.
Dr. William A. Curtin,	Syracuse, New York.
Dr. S. A. Czarra,	Cincinnati, Ohio.
Dr. J. D. S. Davis,	Birmingham, Alabama.
Dr. F. B. Downs,	Bridgeport, Connecticut.
Dr. J. A. Ellegood,	Wilmington, Delaware.
Dr. J. W. Ellis,	Owensboro', Kentucky.
Dr. S. L. Elsner,	Rochester, New York.
Dr. H. B. Ferris,	New Haven, Connecticut.
Dr. George M. Fitz,	Cambridge, Massachusetts.
Dr. C. A. Foster,	Washington C. H., Ohio.
Dr. William A. Fox,	Washington, D. C.
Dr. P. J. Gibbons,	Syracuse, New York.
Dr. T. V. Hammond,	Washington, D. C.
Dr. P. A. Harris,	Paterson, New Jersey.
Dr. Frank Hyatt,	Washington, D. C.
Dr. W. E. Ireland,	Washington C. H., Ohio.
Dr. C. G. Jennings,	Detroit, Michigan.
Dr. H. L. E. Johnson,	Washington, D. C.
Dr. T. C. Kennedy,	Shelbyville, Indiana.
Dr. O. T. Osborne,	New Haven, Connecticut.
Dr. C. J. Orr,	St. Louis, Missouri.
Dr. N. S. Lincoln,	Washington, D. C.
Dr. B. C. Loveland,	Clifton Springs, New York.
Dr. Belle J. McDonald,	Wardhaven Junction, Long Island, N. Y.
Dr. G. R. McDonogh,	Toronto, Canada.
Dr. C. J. MacGuire,	New York City.
Dr. F. S. Milbury,	Brooklyn, New York.
Dr. F. P. Morgan,	Washington, D. C.
Dr. Wolfred Nelson,	New York City.
Dr. P. P. Parrish,	Hot Springs, Virginia.
Dr. D. S. Perkins,	Cleveland, Ohio.
Dr. W. A. Pusey,	Chicago, Illinois.

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Dr. H. A. Robbins,	Washington, D. C.
Dr. James T. Sothoran,	Washington, D. C.
Dr. Z. T. Somers,	Washington, D. C.
Dr. T. L. Stedman,	New York City.
Dr. F. C. Thayer,	Waterville, Maine.
Dr. John Van Rensselaer,	Washington, D. C.
Dr. John Walter,	Washington, D. C.
Dr. J. P. West,	Bellaire, Ohio.
Dr. M. W. Wood, U. S. A.,	Washington, D. C.
Dr. H. C. Yarrow,	Washington, D. C.

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RECAPITULATION.

	Members.			
	Active.	Honorary.	Corresponding.	Present.
American Ophthalmological Society..	120	1	--	36
American Otological Society	85	3	--	29
American Neurological Association ..	87	23	--	36
American Gynecological Society	90	29	--	50
American Dermatological Association	38	1	--	22
American Laryngological Association	60	1	14	30
American Surgical Association.....	80	24	--	41
American Climatological Association.	116	2	--	42
Association of American Physicians .	97	4	--	62
American Association of Genito-Urinary Surgeons.....	48	--	--	27
American Orthopedic Association....	52	8	20	23
American Physiological Society.....	47	--	--	22
Association of American Anatomists.	104	2	--	27
American Pediatric Society.....	45	--	--	22
	1069	98	34	469
Guests and Visitors	---	--	--	58
Total in attendance	---	--	--	527

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MORPHOLOGY AS A FACTOR IN THE STUDY OF DISEASE.

BY HARRISON ALLEN, M.D.

Not long ago I heard a gentleman congratulate a distinguished professor of Greek on the ground that the subject of Greek literature was finished, that is to say, was one to which from the nature of things no important additions could be expected. Any one not a special student of the classics might be justified in making such a remark. The professor replied that he was indeed to be congratulated, but not on the ground of being devoted to a subject whose limitations were known. He then spoke of important discoveries recently made in Greek texts found in Egyptian papyri and manuscripts used in the manufacture of mummy cases. These investigations had led to entirely new conceptions of the social life of the ancient Athenians and of the characters of some of their leading authors.

In like manner the anatomist who stands on the vantage-ground of medicine is often told that by the cultivation of the last four hundred years his subject is finished—that no new thing can be said of it. Whether or not the human anatomist is to be congratulated, being thus identified, is a matter of opinion. For my part, I venture the statement that no true anatomist would be content for a moment with a science thus limited. Were he so to accept it his energies would be necessarily confined to acquiring what his predecessors had secured in an equal degree. I am sure I represent my fellow-workers when I say that the science of anatomy is awaiting important advances—not indeed by unrolling the palimpsests of tombs, or by the maceration of mummy cases, but by new readings in the infinite book of Nature.

To the physician who is busy with the practical duties of his profession, anatomy is the arrangement of the structures of the adult human body by the standard of undergraduate instruction. This anatomy may be well said to be limited in the sense that Procrustes measured a giant by the length of the bed he was stretched upon; the superfluous parts were lopped off to make a fit. The text-book assumes an ideal which is properly described. The facts that exceed this ideal, or otherwise disturb it, are ignored unless they are needed by the surgeon, when they are elaborated just so far as he may dictate. "Gentlemen," said Dr. Barclay, "Vesalius and his fellows were reapers in the great field of anatomy—John Hunter and his

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brethren were gleaners—and we, gentlemen, are the stubble-geese!" Presentation of the facts in this wise was as well-known to the old worthies as they are to us—and yet we contend that moderns can be something better than "stubble-geese." Three sciences have appeared since the days of Vesalius and of Hunter—sciences so vast as to command the time and energies of distinct bands of workers: histology, which yet awaits the discoveries of new chemicals, of apparatus of precision, and of improved means of conservation, for yet greater triumphs; embryology, which, while long claimed by the physiologist, is returning to the anatomist as his rightful inheritance; and general morphology—the most important of the three—which embraces all animated structures as parts in a scheme of philosophy.

I have employed the word morphology, for morphology is anatomy writ large, as the heading under which my remarks may be included; as one least liable to misinterpretation; and I would place within it the subject of descriptive anatomy as a conventional and unimportant part.

I confess to a feeling of impatience that the science of anatomy is still commonly taught pretty much as it was a century ago, and I cannot refrain from expressions of regret that these methods have not undergone change even in the centers which have witnessed important revolutions in other departments. The demands of the surgeon still dominate the lecture-hall. No fact that cannot be used in the clinical amphitheater is thought to possess value; the forces for the education of the medical scholar are unused; the powers of observation of the practising physician are not stimulated; the outlines of method by which the science of morphology can be extended by experiments unthought of; while the opportunities of accumulating at the hands of the physician materials to be used in the explanation of phenomena of organic structure acting perversely, or serving as the causes of diseased action, are neglected.*

* Men who have no sympathy with anatomy and physiology are apt to declare that these sciences have no connection with clinical work, and are thus absolved from considering their claims. A physician interested in the study of speech-sounds, attempting to connect this study with clinical methods, is told by leading physicians that such work is not medical but physiological. Another physician photographs the larynx in the living individual and reports his work before the laryngological section of an international medical congress, when the chair expresses his opinion that such work is not worth the doing, at least that it is not laryngological. The medical man who rises on the floor of a medical society to use the facts of the shape of the thorax as an aid in the study of the predisposition to pulmonary tuberculosis is declared by the chairman to be out of order in a discussion of the etiology of that disease. A member of a pathological society who proposes to read a paper on the anatomy of the cerebrum in connection with localization of diseased action is crowded out by a combination of the members on the ground that a pathological society is a society of clinicians and not of anatomists.

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It is true that these and similar facts are in the possession of advanced clinical workers, but they often appear under heads not suggestive of their exact position in knowledge. Many important considerations are scattered through hospital reports, treatises on practice, or the transactions of pathological societies. Sometimes they are veiled under misleading titles. The actions of muscles have been held to be always corollaries of the descriptions of the muscles themselves, indeed to be logical deductions from the formulæ of their origins, insertions, and nerve-supply. Yet these facts are wrested from anatomy under the name of physiology of movements. Such a classification would bear to physiology the relations which obtain between the structure of bone and the dynamics of the skeleton, the studies of the eruption and succession of the teeth and the rates of growth of organs. Teratology is generally cultivated by obstetricians, and, if students of medicine were taught this subject it is as likely as not that it would be undertaken by the chair of obstetrics, and thus a purely morphological series of demonstrations be not credited to the department of anatomy.

It is evident that physicians who become interested in these and allied subjects obtain their knowledge, so far as it relates to the elucidation of medical problems, after their academic education is completed. Is it not entirely proper that they should be carefully instructed in them before graduation? Is it not patent that the difference between the imperfectly educated and the accomplished physician can be determined by the use of the test of being able to interpret morphological factors in the study of disease, as well as by any other test of efficiency?

Empirical knowledge never leads to true conceptions. The basis of just lines of association must be those established by nature. In a philosophical plan there is no such thing as a series of diseases in the sense of a sequence of organic entities except when these entities have been first established. In a word, diseases are not related to each other but to their causes. Etiology is the single factor that yields conditions which can correlate. Tissue-changes furnish truer guides than function-changes. A character is a more reliable datum than a trait. A description of an organ or part is a more acceptable contribution to knowledge of classification than that of a habit. In medicine we can, in like manner, say that a sign is better than a symptom, or, what is best of all, elaborate experimentation combined with anatomical observations on the dead are worth all that is possible from empirical observations on the living. In clinical work demonstrations are often lacking; tests may be absent. Indeed, the physician prac-

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tically acknowledges that he never proves anything, as he refers his etiology to the chemist and the bacteriologist. I take the position that he should refer to the morphologist more problems than he is inclined to do.

It is acknowledged that chemical conditions underlie texture, and it is a matter to be regretted that the morphologist gains so little by this association of truths. Not, indeed, that the knowledge of chemical changes in tissue do not enter into his problems. He is well aware of the close connection between the chemical composition of teeth and their forms, of the great influence that the beginnings of calcification of the triangular cartilage of the nasal septum exert over the shapes of the nasal septum, as well as the immense range of changes initiated in gouty and rheumatic conditions. Exact relations are shown to exist between the chemical constitution of the fluids and the shapes of some of the organs. These changes, it is true, are often unimportant to the individual, are peripheral, and apparently of no special significance. But the general shape of the gouty tooth is, so far as it goes, distinctive, as also is the fact that the lower incisor teeth are altered slightly in their relations one to another. Sir Dyce Duckworth, in his treatise on gout, states that "buck-teeth" are not uncommon, one of the lower central incisors being thrust out of rank, as was first noticed by Laycock. I have observed these characters in a large number of cases, and am confirmed as to their correctness by dental surgeons.* Fagge† believes that the teeth are especially noteworthy in persons of gouty habit. They are, as a rule, well developed, with strong, hard enamel, which is rather yellow in color. They resist decay, and are firmly set in their alveoli. In time they become more or less worn down, so that the pulp cavities begin to be visible. Pye-Smith quotes the authority of Mr. Moore for the facts that this condition may be caused by that formation of jaws which gives an edge-bite, also in cases in which the enamel is thin on the summits of the teeth.

In like manner the nails, which are so closely associated with the teeth in all morphological characters are of value in studying the gouty diathesis.‡ The nails, according to Duckworth (l. c.) "are observed to be coarse and fibrous, striated and fluted, or lined vertically, in gout. This peculiarity is well marked in most persons of the arthritic diathesis, and hence is found in rheumatic persons. The

* See "The Characters of the Teeth in Persons of the Arthritis Diathesis." Journal of the Odontological Society of Great Britain, 1883, p. 193.

† Prin. and Prac. Med., second edition, 490.

‡ Two hundred years ago Sir Thomas Browne wrote that "tempermental dignotions and conjecture of prevalent humors may be collected from spots on our nails."

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nail-substance is apt to grow thick and brittle, and especially so after attacks of gout. The nails may be shed after several local attacks. The transverse depressions, described by M. Beau and Dr. Wilks, are seen in due time after gouty as after other illnesses, indicating a temporary failure and depression of nutrition of the whole body. As an entire nail takes six months to grow, the site of these furrows indicates the date of the past illness with singular exactness. Sometimes a white line marks the attack."

If among wild animals varieties were found to vary as much among themselves as do gouty and non-gouty persons, zoologists would be justified in defining therefrom subspecies. Why should not gout be defined as well by the results of the chemical compositions of the fluids as by these compositions themselves? Indeed, biological science teaches us that morphological data are the most reliable, and it were better to define gout and rheumatism by the peripheral characters, even if we do not derive therefrom any notable practical advantage.

So long as we study diseases, as distinguished from their causes and effects, we are empirical in our methods. We hear much of the "science of medicine." Surely it is a "so-called" science when it deals only with clinical phenomena. No medical center which is denominated by a clinical element (I am anxious here to recall that by clinical work is meant literally the bedside work, or its equivalent), the study of the patient rather than the causes which evolved the patient (or the possible study of the body of the patient, if he should be so unfortunate as to die), can be said to be an empirical one, and history shows it to be singularly liable to be clouded by speculative opinion. I respectfully submit that the theories of the humorists, the solidists, or any one of the many theories which have agitated the profession (the hosts of spectral forms that surge up to the line of blood in the limbo of medical philosophy), would have been impossible had the system of medicine been broadly based upon morphological data. But such a basis was not feasible until the organic sciences were themselves formulated. The great reputation of Boerhaave rests upon his recognition of chemical and anatomical data as aids to the study of medicine. The amount of anatomical reading he recommended his pupils showed a knowledge of anatomy which has probably never been approached by any of the authorities in clinical medicine since his day.

The changes in the shape and composition and use of a part of an organism constitute a variation—that is to say, an exception to a rule which has been formulated concerning this shape, composition, or use. The detection of these variations, and the propositions upon which

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they are based, constitute one of the chief occupations of the morphologist.

The difficulty of proving the value of minute knowledge of the muscle-fibers of the body is very great, for anatomists so rarely have an opportunity of making the necessary demonstrations. Let me illustrate a few of the very probable etiological factors in such conditions as esophageal pouch and heart-failure. Esophageal pouch is a yielding of the muscle-wall of the pharynx as it joins the esophagus in individuals who possess an insufficient number of the circular fibers of the lower pharynx or the beginning of the esophagus, which will in rare instances have developed an invagination of mucous membrane at the line of fault. This is a simple example of the manner by which a minute anatomical variation may become a factor in the etiology of disease. It is well known that the staying power of athletes relates more to minute inherited peculiarities in the heart than to any other group of conditions. Let a number of men subject themselves to the same physical test—it may be a walking-match or a swimming-match—it will be found (all things remaining equal) that the man with the best heart carries the day. May we not suppose that such a heart had an exceptionally firm closure of the foramen ovale and the ductus arteriosus? That it possessed powerful chordæ tendineæ? That the pulmonary artery was free from the slightest degree of stenosis? What is true of the heart is true of the muscles of the limbs. The differences that serve to mark the musculature of the mammalia are often minute, and not of necessity of the same functional importance as those of larger size, yet doubtless have significance in adopting the animal to special conditions of environment.

In an exact sense, the structure of an animal may be said to express the resultants of certain forces. In a sense less exact, the work of the anatomist can be said to be that of analyzing the results of these forces—as one can determine from the shape of a cedar tree, whether it possesses a tall stem and conical masses of leaves, as is seen in a cultivated piece of protected valley, or whether it is short in stem (a mere brush), and with ragged foliage irregularly dispersed over twisted boughs, as one finds it on wind-dunes at the seacoast. But we cannot fix all the conditions. We know only in a general way that trees grown in localities contrasted by the degrees of violence of the wind will show correspondingly great differences in shapes. In order to accurately assign the work of the various agents, we must know everything of the temperature, degrees of humidity about the trees, the nature of the soil that nurtures them, and the habits of insects and birds that frequent them, to say nothing of the innate

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disposition that the trees may possess to naturally select from their own economy what is best for their welfare. So it is with the causes of variations in anatomy. In a general way the variations themselves are held to be the results of a few all-pervading forces (as a rule, nutrition modified in some way from what is ordinarily met with, plus the trend of heredity), and these are inscribed in the body as dynamic energies may be recorded on a kymographion. But how inadequate such a record becomes is evident when we study any of these factors by the standard of the gross changes excited by disease. Disease in a morphological sense is the expression of forces operating inordinately or eccentrically. Yet how completely our systems fail when we attempt to explain them by the tests of nutrition or of heredity! So when the anatomist is told that his science to a great degree is one of record and not of thought, the answer may be given, properly enough, we think, that the reason lies in the inadequacy of knowledge of the causes of which structures are simply the effects. If the general clinical observer will give the anatomist more data as to the causes of congenital idiocy, the anatomist will be able better to explain the appearances he notes in the dissection of the idiot. If the ethnologist will explain the meanings of the divergences and contrasts of race, the anatomist will give a more philosophical description than is now possible of the differences between the structure of a Hottentot and that of a Northern European.

Thus we may say that in the study of anatomy it is not so much the fact but the import of the fact that is essential. Anatomical facts are like letters of the alphabet, their chief uses consisting in combination for the expression of ideas. Let us suppose that, instead of twenty-seven characters, the alphabet was made up of a thousand, would the conclusion be modified that the thousand signs were still to be regarded as aids to expression? The significance of anatomical facts lies not in their number or arrangement, but in the mind that guides the scalpel and the forceps. A short time ago Drs. Chittenden and Sterling instituted an inquiry into the shapes of the teeth and jaws in feeble-minded children. After completing their work it was deemed desirable to make an examination of the same parts in an equal number of normal children. In the attempts to find such subjects in institutions where the examinations could be conveniently conducted, it was found after proper investigation that no establishment for the reformation of children or the care of orphans would meet the requirements of the case, for such children were not normal —in reformatories, because of the possible inheritances from the criminal classes, and in the orphanges because of the loss of at least

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one parent before the time of the average longevity, introduced elements of inheritance of disease (syphilis, tuberculosis, scrofula, cretinism) which invalidated any claim that might be made that the children were normal. Thus in a matter apparently so simple in essentials as the shapes of the teeth and jaws, four unknown propositions in the study of the morphology of childhood arose:

1. What are the shapes of the teeth and jaws in the normal child?
2. What are they in the feeble-minded child?
3. What are they in the children of our reformatories?
4. What are they in orphans?

Is it not a fair assumption that similar questions can be raised with almost all the systems of the body?

Sir James Paget has lately urged the examination of varieties in different examples of the same disease after the manner of accurate botanists and zoologists by the observation and record of the larger external characters and indications in the human body. Among the special subjects outlined he names the following: 1st, varieties of diseases usually regarded as species, and so arranged in nosologies; 2d, the effect of mingling of diseases by hereditary transmission or otherwise.

While the method is certainly a gain over accepted clinical methods, we venture to assert that the study of etiology is not satisfactory without experimentation. Jenner tells us that John Hunter was in the habit of saying to his pupils "Do not think: try." If we are not careful we may be misled as to the meaning of this saying. We all know that Hunter was always thinking and trying. His dictum is paradoxical unless it be thus read: "Do not stop at the thought; try the thought by the test of observation or experiment."

A zoological station is a place where experiments are conducted in the study of the structure and relations of animals. An anatomical station must be the same if anatomy is to take its place among the progressive biological sciences. Observation has been shown to be insufficient to interpret natural processes. We can observe the effects of an experiment of nature; we can imitate such an experiment; but we cannot ignore the method by which effects are produced. Bates discovered in the valley of the Amazon the exact conditions under which a given species of butterfly existed, so that in studying its varieties he may be said to have witnessed the creation of a new species. Every thriving organism can be said to be results of Nature's successes in her experiments, every languishing one a result of her failures.

Let me in conclusion call your attention to a subject which, while

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resting on biological tenets, is of professional interest. It is that the anatomical material coming under our notice is almost always that of a highly domesticated,* or, as one may say, over-acclimated nature, and that in order to study it with advantage we must recall what is in common between man and the animals (both in a wild and a domesticated state) about him. Not that savage man is of necessity a primitive man; he is often a degraded, a degenerate man. The ranges of anatomical structure are on that account all the more interesting. The human anatomist should hold to the hypothesis that studies in degeneration are really studies of a character so closely allied to those that are morbid that the distinction in a system of knowledge cannot be made. Our acquaintance with man as a domesticated animal, as a degenerate animal, and our ignorance of him as a wild and primitive animal, cause the morphologist to regard human structures with the same sort of interest that the botanist entertains for those plants which have been cultivated so long that he has lost knowledge of the typical forms of the species. Man has doubtless undergone changes in his organization resembling those which exist in a domesticated animal when his organization is compared to that of an ancestral form. Can the anatomy of such an animal be said to be closed? Should we not rather humbly acknowledge that we know little of a subject at once so vast and so difficult?

Dr. F. P. Henry has shown in his classic papers on anemia† that the red blood-corpuscles in this disease approach those of the lower animals in many if not all their characteristics, namely, in their number, their size, their shape, and the amount of hemoglobin they carry. The number of red blood-corpuscles in pernicious anemia is reduced to a degree that is normal in the cold-blooded animals; the corpuscles are increased in diameter, occasionally nucleated and have a ten-

* The use of the word "domesticated" in this sense needs explanation. The *Century Dictionary* gives the following phrase as a definition of to domesticate: "to reclaim from a state of nature." It also gives the following: "to convert to domestic use, as wild animals or plants." I have used the word in the sense first given. Man is "reclaimed from a state of nature," i. e. he is not available as an absolutely feral subject. But he is not domesticated as dogs and horses are, since, as Darwin (*Descent of Man*, i. 112) expresses it, man "differs widely from any strictly domesticated animal, for his breeding has not been controlled either through methodical or unconscious selection." The effects of living in crowds, on unwholesome food, for numbers of generations, may not be those of domestication; neither are they of a kind comparable to those met with in the wild animal. The essential point I wish to make is this, that many morphological details result from self-imposed restraint, and that these details are to be treated in a manner somewhat the same as when met with in the process of changing a wild animal or plant into a domestic one.

† Philadelphia: P. Blakiston, Son & Co., 1887, p. 116.

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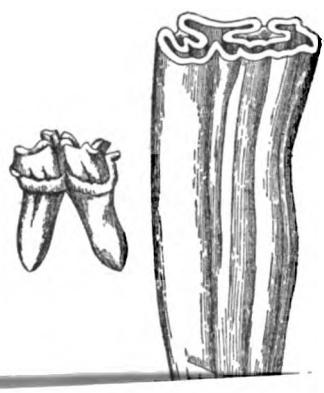
dency to assume an oval outline. The change in the shape and proportion of the red blood-corpuscle in pernicious anemia is an instance of degeneracy mimicking the processes of normal evolution. The type of blood-corpuscles in pernicious anemia does not become identical with that which is normal in a cold-blooded animal, but it resembles the ancestral shape because it has lost the form which is characteristic of its high position in the scale.



While degeneration is a phase of a specialization it is one portraying structures that fail to subserve the highest possible use in the economy, certainly a degree of efficiency lower than that seen in similar structures in animals in a wild state. Paleontology teaches us that the earlier types of horses possessed short crowns to the molar teeth, while recent horses have long crowns. (Figure 1.) These bear to one another a relation quite the same as that discernible in the teeth of man. The best molar type in the human subject exhibits long crowns, while the worst is one in which the crowns are short and, indeed, often scarcely protrude from the gums. (Figure 2.) The term brachidontism and hypsodontism have been proposed to express these conditions; and we can say that while brachidontism is a sign of an early phase of variation in the horse, it is a sign of late phase in man; that in the horse it subserves a relation to the phylogeny of the zoological group to which the horse belongs, while in man it is a sign of degeneracy due to intricate causes, prominent among which lie the pro-

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found depression of vitality arising from inherited struma, tuberculosis, syphilis, or to one or more of these conditions in combination.

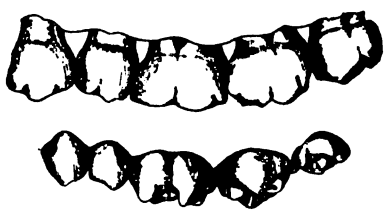

Figure 2. The upper series represents the best expressions (hypsodontic) of the bicuspid and molar teeth of man showing large elongated crowns. From a skull. The lower series represents the teeth of the same name but degenerate in form (brachiodontic) showing small shortened crowns. From plaster cast taken from living subject.

In like manner the phenomena of polycuspidation in the molars relate to the evolution of the group to which a given animal belongs, and the cuspidation may be lost in the attempt to adapt the form to functions of defense. Yet if in the human molar the cusps show a disposition to recede from the quadritubercular to the tritubercular type, the result is a loss to all the previous gain. Nothing good is to be expected to accrue to the individual in which they occur. In the human dental series they are very frequently seen. We can say with certainty that the loss of a cusp in a human molar tooth is associated with decreased initial energy; and that these changes are not due, as in the case in the lower animals, to adaptation to special, and, as a rule, to higher ends.

The same remarks would apply to the proportionate size of the face as compared with that of the brain-case. The long-faced ant-eater is separated by family lines only from the short-faced sloth, and these distinctions have been brought about by the law of natural selection. But the short-faced specimens of the skull of civilized men are the results not of such a law but of the recedence from positions of high specialization secured by the operation of that law. They have tumbled from a lofty position by some profound impression on nutritive forces rather than reverted. Hence they come within the range of degenerated structures.

To what extent structures which are degenerate become on that account predisposed to disease it is difficult to say. But no difficulty exists in obtaining consent to the statement that, taken as a whole, the animal economy resists the inroads of disease in proportion as its vitalities are maintained at high levels of efficiency. It is tenable,

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therefore, that the different parts of the economy which resist disease are also those which are richly endowed with all the essentials of well-being. This line of thought is sustained by my own clinical studies in the diseases of the nasal chamber. Many of the exceedingly numerous variations in the shapes of the nasal chambers are examples of degenerated structures, that is to say they are degraded expressions of anatomical form due to or at least associated with a facial region which is stunted and dwarfed. If such a premise be conceded, then the next thought to my mind is logical, namely, that such structures are of necessity liable to become diseased.

Figure 3. Example to the left of a chamæprosopic (short, squat-faced) human cranium. Example to the right of a leptoprosopic (long, slender-faced) human cranium.

We may assert without fear of contradiction that the skeleton of civilized man in like manner differs from the skeleton of uncivilized man. By civilized man in this connection I mean man as we find him to be in the great centers of population, past or present, where for acknowledged periods of unknown duration he has lived in communities having attained high grades of social organization, such are the people of modern Europe (of America so far as this continent is settled from Europe) and of ancient Peru. Doubtless Asiatic centers must be added to those enumerated. Yet of these, material for study in this country is wanting. If I may be permitted to say in a few words what impresses me most in a series of skulls of civilized centers is the absence of correlation, that is to say I find the disposition

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usually present in crania of wilder types of man for one part of the skull to be in harmony with another part, to be largely absent. The bones themselves appear to be individualized. Let me mention some of these. The occipital bone tends to project between the lambda and the inion. The malar bone tends to elongate vertically, so that the orbit is relatively large ; the ethmoid bone to extend less into the nasal chamber, so that when the middle turbinated bones are sought for in the living subject they are found to be lodged high above the plane of nostrils, and their lower borders are often oblique in position instead of horizontal. The roots of the upper molar teeth tend to be confluent, and the crowns to be elliptical in shape instead of quadrangular. (Figure 3.) Yet not infrequently a small, square orbit may be associated with an elongated jaw, or a short, set jaw with a wide, ovoid orbit. (Figure 4.)

Figure 4. Example of a human cranium which combines the characters shown in the crania of figure 3. The upper part of the face (including the orbits) is chæmoprosopic, while the lower parts (below the orbits) is leptoprosopic.

Perhaps the best single conclusion to be drawn from the study of morphology as a factor in the study of disease is its value to humanity. The scientific study of race in connection with diseased action is almost an unbroken field. When this phase of comparative anatomy shall have been formulated we shall for the first time have a reasonable hope that the subject of human acclimatization, the geographical study of diseases, the causes and motives of migration, and thus indirectly, the history and destiny of man himself may be in shape for elucidation.

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[Since the foregoing address was delivered my attention has been called to a paper read by M. de Giovanni on "The Morphological Examination of the Individual," before the International Congress at Rome. I have also received from Prof. Wilhelm Roux, of Innsbruck, a prospectus of a new publication, entitled *Archiv für Entwickelungsmechanik der Organismen.* This journal will serve as a vehicle for the expression of views much the same as those held in my address. It would seem, therefore, that the subject of morphology, especially in relation to clinical and pathological conditions, is already attracting attention.]

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DISCUSSION ON MORPHOLOGY AS A FACTOR IN THE STUDY OF DISEASE.

PAPER BY

DR. THOMAS DWIGHT,

Parkman Professor of Anatomy in the Harvard Medical School.

Mr. President:—Dr. Allen defines morphology as anatomy writ large. The underlying idea of his paper is to plead for a longer, broader, deeper course in anatomy. The tendinous heartstrings of every anatomist will vibrate in sympathy, producing a symphony of musical murmurs. Perchance in some of us one of the factors will be the moderator band of the ruminant which has left its normal place near the apex of the human heart to cross near the middle of the right ventricle. The expert auscultator can recognize this cord. It is good for his patient that he should know from anatomy that such a band may be present and that its sound is no sign of even functional disturbance. The ophthalmologist is none the worse that embryology teaches him the whereabouts of the hyaloid artery which may persist throughout life. Anomalies therefore, as well as slighter variations are of practical importance. The surgeon needs to know of the fibrous or muscular band which may cross the axillary artery, that the brachial artery may divide high up the arm (very rarely in the lower third) and that the diverging artery, be it radial or ulnar, almost always runs superficially. A third trochanter, which seems much larger than it is, when felt through the soft parts, must not be mistaken for an exostosis, nor a deltoid tubercle of the clavicle for an old fracture. We must not forget that any one of us may at any time meet with a very rare anomaly. I, myself, within a few months have seen a case of complete absence of one kidney, the other being of about the usual size and in its proper place.

But it is not necessary to turn to anomalies to support the need of a deeper knowledge of anatomy for the practitioner. The statics and mechanics of the skeleton, the action of muscles are becoming daily more important to the orthopædic surgeon and to the neurologist. The anatomy of childhood is still almost in its infancy. Though not quite helpless it has not yet made its way into text books, but hides itself bashfully in scattered papers and monographs. A more thorough knowledge would be of great value to the practitioner in

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children's diseases. How interesting and how practical in view of infant feeding is the story of the rate of growth of the stomach in the first months of life and how few know it!

My views of the esteem in which anatomy is held are far less pessimistic than Dr. Allen's. It is but too true that we have many in high places absolutely ignorant of anatomy themselves, who look on its progress with a jealous aversion. But, they are mostly passing off the stage, and they belong to a school of thought that is doomed. On the other hand I look with confidence to the rising lights. I see in the better class of our young practitioners a great respect for anatomy. They have a clearer vision of its sphere. One of the most gratifying incidents in my professional life is the constantly increasing number of those who come to me for information on anatomical problems or for the means of solving them.

Surely after what I have said I shall not be suspected of indifference to the cause if I do not quite agree with Dr. Allen as to the remedy. He would have students thoroughly instructed in the highest anatomy before graduation. I reply that there is no time. As a practitioner myself of twenty-five years standing I am appalled at the amount of knowledge of all kinds which is now necessary for the simplest practice of the profession by anyone who would do his whole duty. I heartily agree with Huxley's saying that whoever adds one tittle that is unnecessary to medical education is guilty of a very grave offense. I would leave the matter to the good judgment and tact of the professor of anatomy. While his teaching should be eminently practical in the ordinary sense of the word, I would have him incidentally point out the explanations of structure from comparative anatomy and embryology and still more insist on the application of morphology to disease. But I would have these things pointed out rather than pursued. I would have him show the possibilities of anatomy which, unfortunately, are impossibilities in the regular course. Thus he will enlarge the student's horizon. It is not necessary to have followed a certain line of research to know that it gives promise of great results. The mind of a student so trained will be in a very different state from that of his comrade who has been crammed with facts from a compendium. Advanced students and graduates are those to whom the highest anatomy will be the most profitable and the more so that they can see for themselves its value which the beginner must take upon trust.

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MORPHOLOGY AS A FACTOR IN THE STUDY OF DISEASE.

BY DR. FRANK BAKER,

Professor of Anatomy in the Georgetown University.

THE paper of Doctor Allen brings forcibly before us subjects that are burning questions with many anatomists of the present day. The study of the human organism, of its structure, its functions, its diseases, of the effect that various agents have upon it, therapeutically and otherwise, must necessarily be a study of life and its manifestations; it is a *biological* science, must be limited and conditioned as other biological sciences are, and have the same general laws of development and correlation. To recognize the necessity of a study of morphology as a preliminary to a proper study of disease is to insist upon this important fact.

Strictly speaking it is impossible to study the phenomena of disease properly without giving due regard to the laws of morphology. Pathology is but exaggerated physiology, pathological processes depend upon abnormal structure as physiological processes do upon that which is normal.

The great impulse given to the study of anatomy by Vesalius and his followers in the sixteenth century has not yet exhausted itself. That great reformer with his fiery zeal and ambition revolutionized medical teaching, insisting repeatedly and always upon the practical aspects of the science. He imagined the body as a piece of mechanism which must be taken apart to be understood. Every muscle must be dissected and its mechanics discussed, every nerve known, every vessel displayed and named. The anatomist became famous in proportion as he was a good dissector, as he had succeeded in unwrapping the structures from their envelopes of connective tissue. It became desirable to have certain fixed elements in the science. Hence there arose a disposition to regard the human body as a completed structure of a definite character, to make descriptions of it that could be easily put in a book so that students could learn them. The countless variations of all kinds that occur in every body were put aside as unworthy of discussion or as annoying abnormalities. The amount of detail involved in a description of the body became enormous, especially as it was not lightened by any attempt to ascertain the laws which produced it. Such was the Vesalian anatomy which has prevailed up to the present time and which has performed great services for mankind.

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Its error consists in considering the body as an essentially fixed organism subject mainly to mechanical laws. From this view arises the notion that anatomy is a limited science, that we are rapidly approaching its bounds and that soon there will be nothing more to investigate. Even a cursory examination of the history of the science shows how baseless is such an assumption. Vesalius knew the anatomy of the heart as well or better than Harvey, he even performed vivisections, yet with all his wonderful power of vision and zeal for his work he did not discover the circulation of the blood. Gall knew the gross anatomy of the brain, but was unable to properly interpret it, considering the great white bundles of the corona radiata as the essential "organs" of mind and quite overlooking the importance of the cortical gray. Schwann discovered that the human body was composed of cells, but he believed that they were generated *de novo* in the liquids of the organism. Gerlach discovered, as he thought, in the spinal cord and brain an intricate network of nerve fibrils that he believed to be the *sensorium commune* where all impulses from without were gathered and sorted, and from which they were reflected upon the motor nerves. This has quite recently been shown to be totally erroneous. Not a day passes but some new discovery or some important generalization is made in anatomy. The books on the nervous system written even five years ago are now to a considerable extent obsolete, so great are the advances of our knowledge within quite recent times.

There has in fact never been a time when it was possible to fix any limit to the field. Whenever investigators imagine themselves at the end, a new series of developments appears showing intricacies beyond intricacies yet unfathomed. Vesalius may have supposed the task was ended when every part had been carefully and skillfully dissected and described. Yet Eustachius at the very same time was demonstrating that by macerating organs they could be resolved into a series of separate tissues. Bichat may have supposed that a complete analysis and classification of these tissues would give the final touch to our knowledge, but only a short time after his untimely death they were resolved into their component elements and the theory of the cell was evolved. Schwann may have thought that he had arrived at the final unit when he reduced all living organisms to cells, which he supposed to be simple vesicles containing fluid and a kernel or nucleus, but the more we have studied the cell the more convinced we have become that we are only at the threshold of our knowledge concerning it. Instead of the simple body conceived by Schwann we find it to be a highly complex structure containing organs and apparatuses

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of its own which are very far from being fully examined or understood. Instead of a uniformity it seems that we have a considerable diversity, and it is even probable that we shall have to abandon the theory that the cell is the final unit. The cell theory of to-day is very different from that of Schwann and of Virchow's Cellular Pathology. There are conflicting theories by the score, and we hear of micellæ and biophors, ids and pangenes as the ultimate elements, beyond our present powers of vision to make out, but manifesting themselves by various phenomena that are as yet by no means fully elucidated. In fact, as the ultimate phenomena of structure are, and probably ever must be, beyond any means of demonstration by vision, it is plain that there can be no limits assigned to the investigations of anatomy.

To the modern biologist the human body is by no means fixed and invariable. The product of forces that with all our efforts we but imperfectly understand, it is incessantly changing and varying as those forces change and vary. Even within the limits of health it is highly improbable that the cells of the body remain the same for any appreciable length of time. If our inspection were sufficiently minute their structure would be seen to be as unstable as the drops of the ocean, swept to and fro by the great tides of life. Every movement of the body, every pulsation of the heart, every thought of the brain brings about some slight change, often imperceptible, it is true, but by cumulative effect becoming notable and important. In disease, when the action of those forces is greatly accelerated, retarded or otherwise modified, corresponding increments of morphological change ensue.

The human body has been studied far more than any other organic form, perhaps quite as much as all others put together. There is no lack of knowledge of facts, but for the ordinary students the details are not classified and arranged as the results and sequences of morphological laws. Our arrangement is as yet empirical, and we are laboring under the same difficulty that the botanists were, inasmuch as we are piling up details upon details without really understanding what they mean or seeking to discover the laws which correlate and control them. The muscular system is described very much the same as Vesalius and Albinus described it, the origins, insertions, shapes and relations of muscles being given without any attempt to analyze the laws that govern their formation, attachment, growth and transference, or the causes of the distribution of the muscular masses. Again, the trunks and branches of the arterial system are studied without considering for a moment the rationale of their development, their origin from a network of amœboid connective tissue cells, and their growth

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from the periphery toward the centre of the body. Yet these facts of morphology are important. The moment we conceive the vascular system as an arrangement of connective tissue structures, a thousand facts with regard to the distribution of vessels and the vascular supply of organs become clear. We see why vessels seek the connective tissue septa, why they penetrate organs between their lobes, why they are subject to certain anomalies of arrangement and distribution, why they are reformed in wounds out of the connective tissue of the surrounding regions.

We may even say that inflammation itself, that process so intimately connected with the phenomena of disease, cannot be properly understood without a knowledge of the morphological character of the tissues involved. It seems clear that the main factors in this process are the cells that constitute the vessel walls. Since these are derived from the original wandering cells of the mesenchyme of the embryo flattened out and arranged to form tubular structures, it is not strange that they should revert to their primitive character when subjected to special stimuli. The different phenomena of inflammation appear to be due to such a reversion. The cells of the vessel walls resume to some extent their independent activities, expanding, contracting, so as to permit extravasation of the vascular contents, and finally proliferating, producing other cells that form the looped vessels of granulation tissue and the dense fibrous masses that finally fill in the gaps and make the scars. Morphologically speaking the whole is merely a phase of cell-life in the connective or mesenchymal tissue. I by no means assume that we as yet fully understand this. The life processes of any cell of the body are too complicated for us to analyze. We do not know why some stimuli set certain cells in action while others affect a different set. But it certainly makes it much clearer to know that we have before us a series of processes similar to those which occur regularly in the growth of the embryo. That in many cases microbes or microbic products are a factor in the production of inflammation only adds to the force of the argument. The rapidly growing science of bacteriology is really only a branch of morphology. Microbes are but isolated cells having the same intimate structure, subject to the same laws of growth, reproduction and life as the cells of the body. Indeed it is only by studying the activities of such independent units that an adequate idea can be obtained of the life phenomena of the cells of the organism. From recent investigations it would appear that we have been classing together as white blood corpuscles a number of forms that are quite different in their structure and in their behavior toward staining fluids. The relation of all of these to the

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phenomena of inflammation has yet to be investigated. It can hardly be doubted that important results will ensue.

The whole subject of tumors has, since the celebrated investigations of Virchow, been studied from a morphological standpoint, and it has become evident that in order to understand their causes and their history we must know more about the laws that control cell formation and the growth of tissues. The very fact that the cells of a sarcoma have no analogy with any adult tissue, but represent a primitive, embryonic form not normally present in the matured body, is of the deepest interest to the morphologist.

As the coral reef is built up by the combined efforts of myriads of zoophytes so is the human body built up by its cells. The gross variations in form must depend finally upon these minute cell activities. These variations may be of all grades, from the slight and insignificant changes in texture that we recognize as individual peculiarities up to the striking deformities treated under the head of teratology. A proper consideration of these is of as much importance to pathology as to normal anatomy. Any of them may be the initial or secondary cause of some diseased action.

The morphologist studies these variations as indicating slow modifications of the form of the human body. For it is well known that like all other living things the body of man is slowly changing, that adaptations are always going on within it slowly fitting it more and more to its environment. Organs disused for a long period of time atrophy and are replaced by adaptations better suited to sustain the activities of the moment. Thus there are constantly left behind a considerable number of structures that have ceased to be of any considerable value, organs that are properly termed *vestigial;* and as they no longer represent a proper adaptation to the environment they are fruitful causes of disease.

Among such may be noted the vermiform appendix of the cæcum, the rarely seen Meckel's diverticulum of the intestine, the thyroglossal duct (by which the thyroid gland formerly discharged a secretion at the base of the tongue), the persistence of the primitive neutral form of the genital eminence producing hypospadias, the persistence of the choroidal fissure of the eye, producing coloboma.

A large mass of anatomical evidence shows that the erect position which man alone of all vertebrates possesses, was gradually acquired. I have reviewed this evidence elsewhere and will now merely mention that it is of great force in its cumulative weight. What especially concerns the present discussion is that in many respects the body has not yet become perfectly adapted to the erect pos-

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ture and that many causes of disease may be traced to this maladaptation. The shape of the pelvis has necessarily become modified in order that it may support the weight of the viscera, yet with all this that weight is often the cause of hernia, and besides, the contraction of the pelvis necessary for support prevents the expansion necessary for the passage of the child's head in parturition. Consequently the pelvis of the civilized woman of to-day is less adapted for easy parturition than is that of her savage sister of the Australian bush, or her savage grandmother of the stone age. It is true that by the thickening of the fascia of the lower abdominal wall some attempt is made to remedy these defects, but the passage of the spermatic cord and the round ligament greatly weakens that support. The frequency of uterine displacements and the tendency of concretions to gather at the most inert portion of the bladder are both consequences of the erect position. Many of the abdominal viscera show signs of maladaptation. The cæcum and the ascending colon are not favorably situated for carrying off masses of fecal matter, the pouches and folds of the peritoneum are not arranged so as to best avoid the entangling of the intestines, the heavy liver instead of being suspended from the spine has become slung from the diaphragm which must therefore, to support the weight, acquire strong connections with the pericardium and the fascia that supports the heart, thus producing below and on either side of each lung strong unyielding walls of fascia that restrict its growth and expansion far more than it is restricted in quadrupeds. The heart itself works to much greater mechanical disadvantage, having to pump the blood to a much higher level and against far greater hydrostatic pressure. From this it can hardly be doubted comes something of the greater tendency of man to affections of this organ, and especially to some of the special consequences of these affections such as ascites, anasarca, venous congestion, etc. It has been clearly pointed out that the valves in the veins are not adapted for the erect position, and that varicose veins are in general due to this cause. It will be seen therefore that the study of morphology has in this case indicated the cause of a number of widely different disorders.

No doubt as the science progresses and we become accustomed to consider the phenomena of disease more and more from the biological standpoint many important generalizations will be made. Some already seem to be almost ready for statement. Among them I will mention what may be called the law of stability. It is this: that the stability of a structure varies directly according to the time it has been functionally active in the ascending series of animal life. The structures most readily attacked by disease are those at either end of the

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scale of activity, that have wholly or partially gone out of use (regressive structures) or have recently been added to the animal economy (progressive structures). The original elementary tissues, epithelium and connective tissue, are the most stable; their derivatives, nerve tissue, muscle tissue, glandular tissue are the least stable. We may even adduce certain details. In the nervous system it is well known that disordered function first appears in the higher or more recently established portions. Loss of memory is doubtless nothing more than the loss of function of associative fibres. In old age there is a gradual and progressive extinction of the higher faculties until at last, unless some accidental disturbance supervene, the nervous activities are reduced merely to those necessary for nutrition, and man has become an oyster. In lead paralysis, pen paralysis, the sudden weakness of the knees in fainting, etc., etc., we see groups of organs that have been but lately added, comparatively speaking, first giving away under strain. In certain other cases there is weakness derived from the progressive atrophy of disused organs. Wiedersheim has pointed out that tabetic affections of the spinal cord are especially likely to affect the lumbar enlargement next to the filum terminale, being apparently an upward extension of the atrophy that has been affecting that structure which is, as is well known, the remains of a former extension of the cord. The apex of the lung is another structure very prone to disorders and in like manner represents a reduced portion of the organ which primitively extended farther toward the head.

It has been possible for me to give only a hurried sketch of a few salient points of this great subject. Morphology is throwing light upon a vast number of subjects connected with the domain of medicine, not only upon the causes of diseases, but upon the action of cells, the problems of therapeutics, the very springs of life and the laws that underlie heredity, development, training and education. It is a growing science, one that is destined to a great future, promising much for the elucidation of the highest problems of medicine.

Professor BURT G. WILDER, of Ithaca, N. Y., said:

I regret that I have been unable to prepare a formal paper for the discussion of this subject, but during the past month every moment of available time has been spent in the study of a specimen of great interest. This specimen, however, is directly in line with the problem which Dr. Allen has offered. If I understand his thesis, it is that *structure is a record of function*, and that diseased structure is a

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record of disordered function; more specifically, there is no psychosis without a neurosis of some kind, and the tendency among psychiatrists of the present day, if they can not find in the brain something which accounts for the mental conditions which were observed during life, is at least to assume that it was there. Now, as a biologist primarily, and a neurologist secondarily, and a psychiatrist not at all, I suggest respectfully that that is not a sound method; it is begging the question. This is precisely what we want to know, whether the brain which we study from a person who was abnormal morally, or intellectually, or with respect to capacity, does embody in its gross structure or in its microscopical constitution, peculiarities which are departures from the standard.

The specimen to which I have referred, and which I shall show to the Neurological Association, is the brain of an educated man, a dentist, in the prime of life. I had observed no evidence of insanity unless we interpret as such the fact that he had executed a document dedicating his brain to me according to a plan which I have put in operation at Cornell University. Since however, I have the promise of the brains of several professors, students and others, I cannot admit that this promise should be regarded as *prima facie* evidence of aberration. Nevertheless, within four months after the promise was made, this gentleman not only committed suicide, but in one sense committed it twice. He wrote a letter declaring that he was going to kill himself. He was found lying dead with a pistol grasped in his right hand and two bullets holes in his head not on opposite sides. One was in the middle of the forehead and the other in the right temple. In making the first wound he apparently endeavoured to injure the brain as little as possible. This ball was deflected by the skull, abraded the cortex, and lodged near the precommissure. The second ball traversed the right subfrontal gyre, the callosum and the left central region.

Now this gentleman had informed me that he had a great source of sorrow; he had lost the lady whom he intended to marry within a short time of the date fixed for the wedding; he also had some money difficulties. He was a skillful dentist, and an assiduous student. But after his death, I heard that he had several peculiarities, and that some even considered that his mind was disordered.* It might well be supposed that something unusual would be found in the brain of a person, who after putting one pistol ball in his brain

* He had three brothers and two sisters, all living. One of the brothers writes me: "There has never been a suicide or any insanity in our family, nor, so far as I can learn, in the families of either of our parents for two generations back." This in contravention of a report, apparently authenticated, at first too hastily credited by me.

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had the persistence and self-control to put a second ball therein. And indeed it does present a most unusual condition of the fissures. During the past month I have done little more than pore over this brain and the photographs which have been taken of it. This is the reason that I have been unable to prepare a discussion of the paper read this afternoon.*

I should like to add a few words in direct corroboration of Dr. Baker's declaration that the human body is not to be regarded as a completed structure. It is far from being complete in several particulars and I think physicians should consider very seriously whether it is not their duty to hasten its improvement in one of these, namely, by the elimination of that worthless and dangerous heirloom, the appendix of the intestine. This matter has come home to me very closely. In the last two years there have been removed in my neighborhood thirty appendices by one surgeon. One of these belonged to the daughter of a colleague; another belonged to a professor himself,—not even the elect are exempt. This gentleman declares himself in better health than for several years before, and commiserates his appendiciferous associates. From recent surgical literature, it appears that the operation for the removal of the appendix, supposing it not to be in a gangrenous condition, presents no great likelihood of mortality,—that with present precautions it is a comparatively safe operation. It is now four years since I proposed that we should not only vaccinate but also de-appendicize; that the child, at a period of life when time is not valuable, and when it might have the best possible chance for survival, should, with proper precautions and with the consent of its parents, be relieved of this organ which later might occasion great loss of time, suffering and perhaps death.

There is a corollary to this. The only mammals beside ourselves which are cursed with the appendix are the four anthropoid, tailless apes, the gorilla, chimpanzee, orang and gibbon, passing over some possible exceptions among the lemurs and marsupials. Should there ever be in this city, or elsewhere, a collection of these very interesting animals, they should have the benefit of the experience through which the human race has passed. In disorders of the abdomen not otherwise accounted for, the appendix should be inspected. In these apes also the appendix is usually longer and more contorted than in man. The fact that the appendix of man is smaller, simpler and hence less dangerous than that of the ape, may have been one element in his success in the struggle for existence, and in gaining superiority over these, our poor relations.

* By a vote of the Congress Dr. Wilder was requested to furnish a report of the above case for publication in the Transactions.— *Vide Appendix.*

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One more point. Dr. Dwight has alluded to the difficulty of including more branches in the curriculum of our medical schools, and to that, say Amen and Amen. As I declared in 1877,* the curriculum is already crowded almost beyond endurance. What we want is to make morphology a subject of common school education, and I predict, on the basis of twenty-six years experience as a teacher, that it will not be long before all college educated persons will know fully as much anatomy, physiology and morphology in the broad sense as is now obtained by the average physician. By this we shall gain two things: for the medical student, a sound basis for the strictly professional branches; for the profession, the intelligent and sympathetic support of a community well informed as to the facts and principles involved in the elucidation of hygienic and biologic problems.

Dr. HARRISON ALLEN, of Philadelphia.

I have little to add in conclusion except to express my gratification and my thanks to the gentlemen who have so fully discussed my paper and entered into sympathy with it.

Dr. Dwight used the term "pessimistic." I should be sorry to be considered pessimistic. I have no ground for being pessimistic. I believe that in the University with which I am connected, students will have the opportunities which I have mentioned. In most institutions I regret to say the standard of anatomical teaching is practical in character and not the one which is most useful as a discipline of the mind.

How are we to obtain the best results in teaching morphology? I agree with what has been said as regards the crowded condition of the curriculum and I would not add anything to it. I heartily approve of what Professor Wilder has said; we should push instruction in anatomy and chemistry back to the earlier years in general education. The student is receiving instruction now, that he should get in the college course. All that he now secures is rudimental. The professor of Anatomy should deal more with such problems as I have referred to and the student must be prepared to understand them. The chemical instruction which he now receives at the medical college should be available at a much earlier period. In my judgment valuable time is lost to the medical student in his studies in rudimentary chemistry and physics. These matters should not

* Should Comparative Anatomy be included in a medical course? Introductory Lecture at the Medical School of Maine.—*N. Y. Med. Journal*, xxvi, 337–379.

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go into the medical course at all. If the student were properly qualified that time would be saved. The time thus gained would be given to anatomy. After all, these are matters of detail and could be easily managed if faculties were in sympathy with the ideas I have endeavored to promulgate. Unfortunately they are not. Dr. Baker dwelt upon histology. I did not go into that subject, yet as I understand it, my position is strengthened by the one he takes. The mechanical disadvantages of the upright position explain conditions that the surgeon is constantly studying. The etiology of many lesions depends upon this general theorem, and the study should be part of every anatomical course no matter how urgent become the appeals to the attention of the student in directions other than those that are strictly morphological.

But we must not forget that, even if the method of teaching in the class room or lecture hall remains unchanged, much can be done by giving advanced students opportunities of inspecting specimens appropriately arranged in museums, or, what is better, in investigating the structure—under varying phases of nutrition—of *living animals.*

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CHEMICAL, PHYSICAL AND BACTERIOLOGICAL STUDIES UPON AIR OVER DECOMPOSING SUBSTANCES, WITH SPECIAL REFERENCE TO THEIR APPLICATION TO THE AIR OF SEWERS.

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Mr. President, gentlemen of the Congress: the subject set for discussion this afternoon, on which I have the honor to speak, is one that has attracted attention from the medical and lay world for a number of years, and opinions upon it are still at variance. When I accepted the invitation of the President of the American Climatological Association to present a paper at this meeting based upon experimental studies relating to the sewer gas question, I little realized the manifold ramifications of the task I had consented to undertake, and if the results of my efforts to obtain more light upon this important subject are not conclusive, and appear rather as indications than actual demonstrations, I look to your indulgence, and trust you may realize that a part, at least, of the shortcoming is attributable to the nature of the problem that has been attacked.

I shall not trespass upon your time with a recital of the opinions that prevail with regard to the influence of the air of sewers, drains, and cesspools in the production of disease; that you will hear from the speaker who is to follow me in the thorough way so characteristic of his writing, but shall ask your attention to a consideration of the chemistry, bacteriology and physics of the air of these localities.

A review of the literature can but leave the impression that many of the opinions advanced from time to time upon these aspects of the subject are manifestly the outgrowth of intuition, or are ideas not based upon exact methods of investigation. From such sources we might be led to believe that the air of sewers is a mysterious mixture of poisonous, gaseous products of decomposition, holding in suspension innumerable bacteria capable of producing all manner of disease, and existing under a tension that enables it to press outward in all directions, and especially into houses that are in communication with

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the sewer. On the other hand, opinions that are based upon the study of this problem by precise, scientific methods demonstrate the inaccuracy of such views at almost every point that the subject has been approached. We are now tolerably well acquainted with the nature of the air of sewers, thanks to the efforts of a few who have endeavored to learn the truth on the subject, and we know that as ordinarily found it does not differ very conspicuously from the air we are accustomed to breathe. As a result of the numerous and careful studies upon the chemistry of such air *in situ* by deClambray,[1] Lethby,[2] Nichols,[3] Miller,[4] Russell,[5] Beetz[6] and others, we learn that it is ordinary atmosphere with which are mixed certain gaseous products of decomposition; and that the most conspicuous chemical difference between it and pure air is that it is usually a little poorer in oxygen, and richer in carbonic acid, ammonia and sulphuretted hydrogen than is outside air. It was demonstrated by the investigations of these observers that the proportion of oxygen in the air of all sewers examined by them was never found to be lower than 17.4 per cent. by volume, with one exception, and this occurred in a choked arm of the notorious sewer Amelot in Paris where the reduction of this gas reached to 13.8 per cent. by volume. The same observers found that the proportion of carbonic acid in the air of these sewers fluctuated between 0.12 per cent. and 3.4 per cent. by volume, and that frequently the other gaseous products of decomposition were either not to be detected with accuracy, or they were present in very small amounts.

The studies that have been made by Miquel,[7] Petri,[8] and Carnelley and Haldane[9] upon the air of sewers with reference to its bacteriology are concordant in demonstrating its comparative poverty in bacteria, their number becoming worthy of consideration only under such conditions as splashing or active bubbling of the sewage, and even then rarely or never reaching the figures obtained from analyses of the air of the overlying streets or of inhabited houses.

A number of efforts have been made to determine if there exist in sewer air volatile organic matters that could be held accountable for the diseases believed by some to be contracted through the respiration of such air, and while different observers report the finding of such organic matters, the amounts in which they were detected are so small that they can readily be dismissed from consideration.

We have been led to believe, through some of the teaching on this subject, that the air of sewers exists under an active pressure, and tends to force its way into houses in connection with the sewer. Those who have studied the conditions as they are found in sewers have thus far failed to obtain any evidence whatever in corroboration of such an opinion.

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Professor Chandler[10] in his lectures before the Trades School Plumbing class expressed himself on this point as follows: "The common idea that gas in our sewers exerts a pressure to get out is a fallacy; I have yet to find a case to prove it." He continues—"I some time ago had a lot of pressure gauges made which were distributed among the students with the instruction to apply them wherever they could, but there has as yet been no case of pressure reported." He had a vent taken from the street sewer to his laboratory, thinking that then he could get sewer gas whenever he wanted it by simply turning on the stop cock, but no pressure has yet been indicated. He concludes: "The sewers are not tight enough to stand any pressure from gases. They are not even impervious to water."

The well known observations of Rozahegyi[11] upon the sewers of Munich demonstrated that in 172 observations there was on 57 occasions no movement whatever of the air in the sewers; that for 45 times when he found the current of air flowing toward the houses it was in the reverse direction 70 times; and the studies of Santo Crimp[12] upon the movements of the air of the sewers at Wimbledon demonstrated the existence of currents away from the houses on 273 days against 97 days in which it was toward them, and 88 days in which there was not sufficient movement in either direction to be detected by an anæmometer.

These studies upon the chemical, bacteriological and physical side of the subject together with many observations upon the health of those constantly exposed to the emanations from sewage, and other matters in which decomposition is in progress, are certainly not of a nature to warrant the opinion that sewer air is the dangerous factor that some believe it to be.

Among the results obtained by those who have studied the conditions found in sewers by exact analytical methods there does not appear a single conclusive demonstration that the air of sewers stands in causal relation to the diseases for which it has been held accountable.

Are we justified in considering these observations as conclusive? Do the observations made in sewers, as we find them, indicate all that is possible to occur in these places?

Is it possible to decide if the unimportant nature of this air *in situ*, as determined by chemical analysis, results from inactivity of the decomposition in progress in the sewage itself, or, is due to constant and rapid dilution of the products of active decomposition by air from without?

Can we, through laboratory methods, offer any arguments that may be of service in formulating a reasonable view upon the bacteriological

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condition of this air, and its probable rôle in the spread and dissemination of infectious diseases?

Is it possible to demonstrate experimentally on animals that the gases arising from sewage or from other decomposing substances have any direct effect upon the health of the animal, or its ability to resist infection?

These represent some of the problems that we have endeavored to solve by experiments in the laboratory, problems that can only be satisfactorily answered when practically all surrounding conditions are under the control of the experimenter.

No argument is required in support of the statement, that when one enters a sewer or smells of sewage the impression made upon the senses is very different from that obtained from such actively decomposing substances as an infusion of meat, or other bodies rich in proteids, and if sewage is confined in a closed place there is often no evidence to the senses of active decomposition, and one cannot avoid the conclusion that the changes going on in sewage must frequently be relatively inactive, or else, different from those that accompany ordinary putrefaction. I have endeavored to satisfy myself as to the accuracy or falsity of this opinion by parallel experiments made with sewage, and with solutions known to undergo active decomposition. The results of these experiments, will, I think, suffice to demonstrate the relative inactivity of the decomposition frequently in progress in sewage as compared with matters more favorable to the process. They also indicate by their variability the impropriety of preconceived general notions on the subject. We find that with certain samples decomposition is so feeble that after as long as 14 days there will still remain as much as 17 per cent. by volume of available oxygen in the air of the flask and the amount of carbonic acid will have increased to only 1.42 per cent., whereas with other samples in which the sewage is mixed with slime scraped from the sides of the sewer, the gaseous exchange is much more conspicuous, in one case being quite as pronounced as that seen in meat infusions undergoing putrefaction under similar conditions (see Table I). These differences are hardly to be explained through differences in the character of the microorganisms, to the activity of which decomposition is due, for, if we remember the studies of Roscoe and Lunt[13] upon the bacteria of sewage, they demonstrate that the majority of the different species isolated by them possess the property, when placed under conditions favorable to growth, of conspicuously reducing the amount of oxygen in the overlying air, and increasing coincidently the proportion of carbonic acid. The studies of Hesse[14] have shown that this is a prop-

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erty not peculiar to putrefactive bacteria, but is one that is possessed by a number of other forms not normally concerned in putrefaction. Evidently then the limited gaseous exchange that we observe, at least in some samples of sewage, is owing to unfavorable conditions offered by the sewage itself.

From another series of experiments additional evidence of the inactivity of the decomposition in sewage has been obtained. If one connects a mercurial monometer with a flask containing an actively decomposing body, such as an infusion of meat, one observes after a very few hours that a change in the level of the mercury has taken place. There is usually at first a slight depression of the mercury, due doubtless to the primary extraction of oxygen. This is quickly followed by an increase of tension, which, after a few days becomes conspicuous, often reaching in from 6 to 8 days a positive pressure of as much as 120 or 130 millimeters. I have tested two samples of sewage by this means; one consisted only of the fluid sewage as it flowed from the sewer, while the other was a mixture of this fluid with slime scraped from the bottom of the sewer. In neither case was there an excursion of the mercury much beyond what could be accounted for by fluctuations in the temperature of the surrounding air. The results of these observations as compared with a similar experiment with putrifying meat infusion are represented graphically in chart I, and, as will be seen, give testimony in favor of the opinion of Professor Chandler already quoted. In the light of experiments of this kind, conducted under circumstances most favorable to the activities of the factors concerned in causing an increase of gas tension, it is difficult to maintain that the air of so pervious a vessel as a sewer could acquire, as a result of decomposition in the sewage, sufficient pressure to enable it to force itself outward for any great distance.

During the course of this series of studies I have endeavored to detect the presence of the common products of decomposition other than carbonic acid in the air over sewage confined in sealed flasks, with irregular results. From many samples it was possible to obtain evidence of sulphuretted hydrogen, sometimes not in sufficient amount to detect other than qualitatively, while from other samples it was given off in relatively large amounts. In two samples none was detected, although the observation covered a period of six weeks in one and three months in the other case. Ammonia was practically always present, and varied conspicuously in amount. The air over several samples was tested for the presence of methyl-mercaptan, but I did not succeed in detecting it in any, though it was easily demonstrated

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in the air over actively decomposing meat infusions kept under similar circumstances. We have also endeavored to obtain a closer acquaintance with the effect of ventilation upon decomposing bodies in much the same way that Erismann[18] studied the gaseous products given off from decomposing faeces and urine. For sake of comparison these experiments were made upon decomposing meat infusion (50 grams meat to 1 litre of water) and upon sewage obtained from neighboring sewers.

The experiments were arranged as follows: Triple neck Woulff bottles of four litres capacity were one-fourth filled with meat infusion or sewage as the case might be, thus giving a ratio between air space and material of 3:1. Into one neck of the flask was fixed, by means of a rubber stopper, a glass tube of about 5 mm. caliber, this reached to within 2 cm. of the surface of the fluid; into the middle neck was placed a thermometer and into the remaining orifice was fixed a short glass tube also of 5 mm. calibre. Both tubes were provided at their outer ends with hermetically closing stopcocks. All joints about the stoppers were sealed with paraffin. By this arrangement the air was caused to enter the flask by the longer, pass over the fluid and leave by the shorter tube. The air did not bubble through the fluid but passed over it. The entrance to the flask was guarded by an absorption flask containing pumice stone saturated with a 50 per cent. solution of potassium hydroxide, so that on entering the flask the air was free from carbonic acid. Between the exit tube of the flask and the aspirator, the absorption apparatus proper was arranged as follows: the air passed from the vessel containing the infusion or sewage first through two calcium chloride tubes, to dry it; it then passed through two U-formed absorption tubes containing pumice stone saturated with copper sulphate and dried, according to the directions of Fresenius for estimating sulphuretted hydrogen gravimetrically (see Fresenius' Quantitative Analyse, Vol. I, p. 505, also Zeit. für analytische Chemie, 10–75), it next passed through (during the first three or four days) Pettenkofers' tubes containing barium hydroxide solution; these were, however, abandoned and 2 absorption tubes containing pumice stone saturated with 50 per cent. caustic potash solution were substituted and the estimations made gravimetrically. To avoid error from loss of moisture these tubes were guarded at their exit by a layer of granular calcium chloride 4 cm. deep, separated from the pumice stone by a layer of asbestos. The air next passed to a gas meter that registered its volume in litres and thence to a Chapman water pump.

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The Pettenkofer tubes and barium hydroxide solution were discarded for the reason that, where large quantities of carbonic acid are given off, it is difficult to say when the solutions are saturated, and if a second and third tube be added to indicate this point we have no trustworthy means of determining if any, or what proportion, of the precipitated barium carbonate in the first tube is redissolved by the excess of carbonic acid passing through it.

As the aspiration of air over the decomposing matters covered a number of hours between the estimations, usually about 24, and the temperature of the room was constantly changing, it was not thought advisable to reduce the air volumes to normal conditions from such data, so that in this respect there is a slight error in the results, an error that I hardly think could have been eliminated without having known the temperature of each litre, or small multiple thereof, that passed through the apparatus.

In the accompanying tables (II and III) will be seen the data obtained from continuous observations made in this way upon air passed over decomposing meat infusion (Table II) and upon air passed over sewage (Table III). In Charts II and III these results are represented graphically.

As a result we find the amount of carbonic acid given off from sewage is constantly smaller than that obtained from the meat infusion, while at times the proportion of sulphuretted hydrogen from the samples examined became suddenly comparatively high. Whether the large amount of sulphuretted hydrogen given off by the sewage is due to the presence in the samples of sulphur compounds more easily converted into sulphuretted hydrogen than those in the meat infusion; whether in the case of the meat infusion it was held back through combinations that it may have formed with other non-volatile or less volatile substances, or whether the difference is the result of peculiar fermentative processes in the one case that were not in operation in the other, as the observations of Petri and Maasen[16] and of Stagnitta-Balisteri[17] have shown to be possible, I cannot say, the difference is, however, striking, as reference to the charts will show. With the sewage, as with the meat infusion, a sudden but temporary fall of temperature was almost invariably followed by a conspicuous increase in the amount of sulphuretted hydrogen in the air, while the carbonic acid remained unaffected (see tables and charts). This sudden diffusion of sulphuretted hydrogen from the fluid, in which it, in part, seemed to be loosely dissolved, into the overlying air is doubtless to be explained through the difference of specific heat of the two bodies that are in juxtaposition, the sudden fall in temperature favoring diffusion from the

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fluid of high, to the air of lower specific heat. Another point of interest that is brought out in these experiments is that gentle agitation of the fluids likewise increased the amount of sulphuretted hydrogen in the air, but had little effect on the carbonic acid. This is particularly the case with the sewage, where there was always a tendency to the formation of very dense pellicles on the surface, which seemed to prevent, when intact, the escape of this gas from the fluid. The breaking of such pellicles was almost invariably followed by an increase of sulphuretted hydrogen in the overlying air (see Chart III). When the solutions were kept perfectly still, and there was but little fluctuation of temperature, steady aspiration of the air over them showed a pretty constant production of carbonic acid, while the amount of sulphuretted hydrogen would often be reduced to nothing. A summary of these observations shows us that air passed over such an actively decomposing body as an infusion of meat, when ventilation of the chamber containing it is conducted at such velocities that the entire air is changed from 20 to 60 times in 24 hours, carried off as a mean of 23 observations on as many days, 29.96 parts per 10,000, by volume, of carbonic acid, the highest relative amount on any single day being 75.97 parts per 10,000 with the ventilation equivalent to renewal of the air 24 times in 24 hours, and the lowest being 15.1 parts per 10,000 with ventilation at the rate of 19 renewals of the air in 24 hours. When ventilation was reduced to practically nothing, that is, when the total volume of air was renewed only from one to three times in 24 hours, the mean amount of carbonic acid reached the enormous proportions of 682.7 parts per 10,000. Air similarly aspirated over sewage at velocities that insured complete renewal of the contents of the chamber from 12.5 to 80 times in 24 hours gave as the means of all analyses in each group of experiments the following relative proportions :

Group 1—From December 11th to 30th inclusive=8.61 parts per 10,000.

Group 2—From January 10th to 19th inclusive=15.4 parts per 10,000.

The highest relative amount found was at the beginning of group 2, when with a renewal of the air 28.8 times in 24 hours, 43.87 parts per 10,000 were obtained, while the lowest amount found was in group 1, when with a renewal of the air 56 times in 24 hours only 3.31 parts per 10,000 were present. Expressing in the same terms the extremes of the results of analyses made *in sewers* by the observers quoted, we find that carbonic acid was found to fluctuate between 12 and 340 parts per 10,000, while often other bodies were present in hardly

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appreciable amounts. Under all conditions, therefore, not only in closed chambers, but under continuous ventilation (though varying in its rate), the decomposition in the samples of sewage examined, as evidenced by carbonic acid, its most conspicuous product, was usually of much less intensity than the decomposition of substances more favorable to the processes.

When it finally became evident that the supposed poisonous properties of the air of sewers could not be attributed to the substances that investigation had shown to be present, in the amounts that they were seen to occur, attention turned to the probable existence of a substance or substances, not included in these results, that might possess disease-inducing peculiarities. By some these bodies are assumed to be of a volatile organic nature, possibly closely related to the substituted ammonias; by others they are said to be carbo-ammoniacal in nature; by still others to be albuminoid; but in fact little definite is known as to their existence or of their nature. From time to time methods have been devised for their isolation with the result that substances were obtained that gave certain reactions common to organic matters, that is, of decolorizing potassium permanganate, or of being oxidized by it to ammonia. A detailed discussion upon the relative merits and accuracy of the various methods that have been employed would be out of place here. It will suffice to say that in the air over decomposing matters, just as in the open air, the air of rooms, and also in water, there may and do exist substances capable of reducing potassium permanganate, but which are not of necessity strictly organic, or of such a subtle nature that they cannot be easily detected by ordinary methods of analysis. The simple statement, therefore, that something has been found that decolorizes solutions of potassium permanganate, means little or nothing as to the nature of the substance. If, however, a body or group of bodies are found which, when oxidized, result in compounds that we know can come only from materials containing elements characteristic of certain groups, we then have a clue, at least, as to their general character. The oxidation, for example, of organic substances to ammonia leaves no doubt that the oxidized bodies were of nitrogenous nature. Advantage has been taken of this and methods have been devised for absorbing from the air its volatile principles and determining what proportion of them contain nitrogen, both in the form of free ammonia, and of *potential* or *albuminoid ammonia*—that is, nitrogenous matter convertible into ammonia, which latter is the body assumed to possess poisonous peculiarities. Of the various devices that have been recommended for their estimation the one suggested

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by Professor Remsen[18] was selected as open to least objection, in so far as the chemical processes are concerned. It consists in aspirating the air that is to be studied through glass tubes containing granular pumice stone saturated with pure distilled water; these serve as absorbers for free ammonia, ammoniacal basis, and presumably other gaseous nitrogenous matters. When the desired quantity of air has been drawn through such absorbers their contents are transferred to carefully cleansed retorts or distilling flasks containing a measured amount of pure distilled water; the free and albuminoid ammonia of this mixture is now determined by the well known method of Wanklyn, Chapman and Smith. By deducting from the totals now found the amounts of free and albuminoid ammonia previously determined for the water used, the balance represents the amounts of these bodies that were present in the volume of air aspirated through the absorber.

There are many details to be observed in the performance of this method: the flasks and condensers must be absolutely clean and an estimate must not be undertaken until it is possible to obtain from the apparatus distillates that give no reaction for ammonia when tested with Nessler's reagent in nesslerizing tubes 31.5 cm. long and 1 cm. internal diameter. (Recommended by Drown.[19]) The absorbing tubes 13 cm. long and about 8 to 10 mm. inside diam. must be cleansed in a hot solution of sulphuric acid and potassium bichromate, after which is it our custom to rinse in hot water and finally pass steam through them for at least two hours. The pumice stone, broken and carefully sifted until the average size of all granules is about that of a linseed or a little larger, is heated in a covered platinum dish until red hot and kept at this temperature (in our experiments), over night, at least. It is then filled into the absorbing tubes. All rubber tubing, rubber stoppers, glass connections, etc., were cleansed first in boiling water and finally by passing steam through them.

The nesslerization of all distillates and standards did not take place until they had stood, carefully protected from the air by steamed corks, for 18 to 20 hours in order that they might all be of the same temperature (see Hazen & Clark[20]), and it is advisable, before beginning the work, to make up a sufficient quantity of Nessler's reagent to last throughout the entire series of experiments.

The distilled water used for dissolving out the materials contained in the absorbers must be carefully prepared, and in each experiment an analysis of the amount of water used in that experiment must be made, the result representing the correction (subtractive) to be made from the total results of the experiment.

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For further particulars we must refer to Prof. Remsen's original paper. (See bibliography.)

Though objections have been raised to this method because of the extraordinary care necessary to avoid errors, it has, nevertheless, appealed to us as the one most likely to give concordant results when properly manipulated. Like all other methods that have been devised for this purpose, however, it affords no accurate information upon the nature of the substances retained in the absorber beyond the fact that they are or are not nitrogenous and are or are not convertible into ammonia by the processes to which they are subjected.

We have applied this method to the study of the air over decomposing meat infusions and to that over sewage, and for purposes of comparison, to outside air and air of the laboratory. The results have not however agreed with those of other observers using other methods, or with those of Professor Remsen himself using the same method. Throughout, our results have been uniformly lower than we had anticipated they would be. Of 31 estimates made upon the outside air, the air of the laboratory, air from over decomposing meat infusions and the air from over sewage, we have failed to obtain evidence of the existence of more than a trace ef gaseous, nitrogenous, organic matters, other than ammonia, the amounts being constantly so small as to fall far below the permissible limits of experimental error.

Of 8 analyses of the open air obtained from just outside the second story of our laboratory not one showed the presence of nitrogenous organic matter, convertible into ammonia by the method used, though from 35 to 355 litres of air were drawn through the absorber. The amount of free ammonia was so low as to fall easily within the category of a "trace."

Of 5 determinations made upon the air of my own room in the laboratory negative results were obtained in all, though from 31 to 207 litres of air were drawn through the absorber. Free ammonia was present in barely a trace, and in one analysis none was detected.

Of 6 experiments in which air was conducted *over* sewage, confined in a closed chamber, evidence of the presence of volatile, nitrogenous, organic matters was obtained in 3, the highest amount expressed as albuminoid ammonia being 0·004 of 1 milligramme for 36·1 litres; while in the remaining three experiments no trace of such bodies could be detected.

Of 7 experiments made by allowing air to *bubble* through sewage before passing to the absorber, the presence of volatile, nitrogenous, organic matters was demonstrated in 3, the remaining 4 being negative. The highest amount found in terms of albuminoid ammonia was 0·005 of 1 milligramme for 866 litres of air.

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Of 4 experiments in which from 178 to 337 litres of air were aspirated *over* decomposing meat infusion, evidence of organic matter was obtained in all, the highest amount being 0·0172 of 1 milligramme of albuminoid ammonia for 337 litres; while in one experiment in which 143·2 litres of air were caused to bubble through decomposing meat infusion before passing to the absorber only 0·0019 of 1 milligramme of albuminoid ammonia was obtained.

These figures are low, far lower than those given for organic matter in the air by other observers, and yet I am convinced that they are still higher than they should be, the excess being due to errors during manipulation, some of which can hardly be avoided, while others seem explainable.

I account for the discrepancy between these results and those of Professor Remsen in part through the fact that in making the distillations he assumed that all free ammonia was given off in the first 100 c.c. of the distillate that passed over, and that all ammonia that now appeared, after the addition of the oxidizing agent, was derived from organic matter of an albuminoid nature.*

In my experiments with sewage and with decomposing meat infusions, I was surprised to notice that, in the distillation of the contents of the absorbers for *free* ammonia, bodies that reacted with Nessler's reagent similarly to ammonia were often coming off at the end of 100 c.c. of distillation in such quantities that the process was continued without the addition of the oxidizing agent. The result was that in these analyses, particularly, this body often persisted until as late as 350 c.c. of distillation. The addition now of the oxidizing agent (alkaline potassium permanganate) showed that whatever was left in the retort was either not convertible into ammonia, or if so, in very minute traces.

Because of the volatile nature of ammonia, we were inclined to the opinion that this body must have been something that gave with Nessler's reagent a reaction the same as that of ammonia but which was not ammonia pure and simple, differing in its physical properties at the boiling temperature. A series of control experiments, however, demonstrated that contrary to opinion, ammonia, as derived from pure ammonium chloride in the presence of sodium carbonate at the temperature of 100° C., does not all come off in the first 100 c.c. distilled over, but is being given off up to the end of 200 to 350 c.c. of distillation, according to the amount originally present. (See Table IV.) In the same series of control tests a few experiments were also made upon

* National Board of Health Bulletin. Washington, D. C., Sept. 11, 1880, Vol. 2, No. 11, page 518.

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methylamine and trimethylamine, alone and mixed together, and mixed with ammonia, and as the results show, they behave in all essential respects identical with ammonia under the conditions employed.

It is manifest that unless this point be borne in mind a large proportion of the ammonia obtained after the addition of the oxidizing agent, if this be done at the end of 100 or 150 c.c. of distillation, is not of necessity ammonia derived from bodies of an albuminous nature but is ammonia, or substituted ammonias, that could have been obtained without oxidation. It seems, therefore, probable that some, at least, of the albuminoid ammonia found in Professor Remsen's work may have been free ammonia or its substitution products.

An example from one of several of my experiments may serve to make this point a little clearer and to demonstrate the possible error if what has been said is not taken into consideration. On Jan. 8th, '94, 143·2 litres of air were aspirated through a decomposing meat infusion and thence through the absorber. The evidence of ammonia in large quantities was so conspicuous that 750 c.c. of pure distilled water was mixed with the contents of the absorber, instead of 500 c.c., as was more commonly the practice. The distillation for free ammonia was made in fractions of 50 c.c. each, and up to and including the 7th fraction, that is at 350 c.c., there was still evidence of free ammonia being given off, the 7th distillate containing 0·002 mg. The 8th fraction gave no reaction for ammonia. The test for albuminoid ammonia was then made, resulting, after corrections for water used, in the total of—

1.540 milligrams free ammonia
0.0017 " albuminoid ammonia } for 143.2 litres of air tested.

Had, however, the results been calculated upon the assumption that all free ammonia had been given off at the end of 100 c.c. of distillation, it is manifest that all that followed would have gone to the credit of albuminoid ammonia and the apparent amount of organic matter in the air would have been far greater than the case warrants. In short, the results calculated in this way would have indicated the presence of

1.341 milligrams free ammonia
0.2007 " albuminoid ammonia } in the 143.2

liters of air analyzed, in other words, the figures indicative of the presence of organic matter as expressed by albuminoid ammonia would have been more than a hundred times too high; and when the results of all my experiments are compared with what they would have been had I calculated albuminoid ammonia from the end of

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100 c.c. of distillation I find that throughout they would have been from 40 to 100 or more, times greater than they are.

In the same way I propose to explain what I mean by saying that my own results are too high. A few examples will suffice for this.

On Jan. 4, 1894, two experiments were made with air that had bubbled through sewage. In the first, 320 litres, and in the second, 866 litres were tested. I was not aware at the time of the persistency of free ammonia in the boiling solution so that the estimation for albumenoid ammonia begun at the end of 200 c.c. of distillation. On the following day the fractions were nesslerized with the following results:

1st Analysis—1st fraction = 0.352
2d " = 0.060
3d " = 0.012
4th " = 0.0025
} Free ammonia total = 0.4265.

1st fraction = 0.0045
2d " = 0.000
3d " = 0.000
} Albumenoid ammonia = 0.0045.

2d Analysis—1st fraction = 0.440
2d " = 0.120
3d " = 0.024
4th " = 0.006
} Free ammonia total = 0.590.

1st fraction = 0.0055
2d " = 0.002
3d " = 0.000
} Albumenoid ammonia = 0.0075.

Water used in these analyses. Subtractive correction for preceding figures.

1st fraction = 0.008
2d " = 0.0015
3d " = 0 000
} Free ammonia, total = 0.0095.

1st fraction = 0.0025
2d " = 0.000
3d " = 0.000
} Albumenoid ammonia = 0.0025.

It is, I think, probable from these figures, that a portion, at least, of the results given for albumenoid ammonia really represents free ammonia. Just how much it is impossible to say. When this became evident 284.5 litres of air in one and 387.7 litres in another experiment were aspirated through *the same sewage*. The absorbers were mixed with 750 c.c. of distilled water and *eight* fractions of 50 c.c. each were distilled off for free ammonia with the following results:

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1st Experiment—1st fraction = 0.120
2d " = 0.036
3d " = 0.014
4th " = 0.004
5th " = 0.0015
6th " = 0.000
7th " = 0.000
8th " = 0.000
} Total free ammonia = 0.1755.

1st fraction = 0.0039
2d " = 0.000
3d " = 0.000
} Total albumenoid = 0.0039.

2d Experiment—1st fraction = 0.180
2d " = 0.056
3d " = 0.024
4th " = 0.010
5th " = 0.0025
6th " = 0.0015
7th " = 0.000
8th " = 0.000
} Total free ammonia = 0.2740.

1st fraction = 0.0088
2d " = 0.000
3d " = 0.000
} Total albumenoid ammonia = 0.0088.

The 750 cc. of water used contained—

Free ammonia = 0.0143
Albumenoid ammonia = 0.0088
} Subtractive correction for the two preceding analyses.

It is not only seen that free ammonia was coming off as late as the fifth fraction in the first and the sixth fraction in the second experiment, but also that if distillation is continued until it ceases to appear before the test for *albumenoid* ammonia is begun, the figures indicative of the existence of this body become very much diminished.

A comparison of the results of these two experiments, calculated for the same volume of air in both cases will, I think, impress this point more clearly.

In the first pair of determinations there was evidence of albumenoid ammonia for 1000 cu. meters of air in the following amounts :—

1st Experiment = 6.25 milligrams per 1000 cu. meters.
2d " = 5.78 " " 1000 "

While in the second pair, made with air bubbling through *the same sewage*, but with the point already made borne in mind, we obtained albumenoid ammonia in the following amounts :—

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1st Experiment = 0.852 milligram per 1000 cu. meters.
2d " = 0.000 " " 1000 "

The means of the results of each pair standing in the relation to one another as 34:1, an error that we think worthy of consideration.

Still another reason for believing that my results are too high has been brought out in several experiments in which a group of absorbers were used, *i. e.* in which the air passed through a series of two or three absorbers. In these experiments care was taken to prepare all absorbers in identically the same manner and it was not infrequent to find, though in exceedingly small amounts, that apparently more albumenoid ammonia was obtained from the second or third absorber than from the first. An example—on Jan. 12, 1894, 203.8 litres of air were drawn over a decomposing meat infusion and through two absorbers. Analysis of these absorbers gave for the *first* 0.0007 mg. and for the *second* 0.0022 mg. albumenoid ammonia, more than three times as much as was found in the first.

In consequence I cannot avoid the opinion that, though the method has been performed with the greatest care, the results obtained by me for albumenoid ammonia are more apparent than real and that the figures presented for the amounts of organic matter as indicated by the albumenoid ammonia test are in part, at least, due to unavoidable error during manipulation. The results of these analyses are given in Table V, column 7.

In addition to this method I have endeavored to employ the one suggested by Carnelley and Mackey[1] in which the quantity of organic matter is determined colorimetrically by its decolorizing action upon solutions of potassium permanganate, but the results were so unsatisfactory that it was abandoned after a few trials. The color of the standard solutions recommended by them is so intense that errors of considerable magnitude may often exist without being recognizable; moreover, in the air in which we were interested so many reducing bodies, other than those of an albumenoid nature, were present that decolorization of the solution exposed to such air afforded evidence of but little value.

From the studies that we have made, and we consider them a fair test, it must be concluded that we have no good evidence of the existence of volatile, nitrogenous, organic matters in the air other than ammonia or its substitution products. A series of experiments are under way to determine if it is possible to differentiate between ammonia and its substituted compounds in the small amounts that they appear in the air; and while the outlook is promising I hardly think the work sufficiently far advanced to justify positive statements as

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yet. It is manifest, I think, that the observation on the air of sewers *in situ* as well as those here presented, demonstrate that, as compared with other substances, the decomposition that goes on in sewage and the consequent pollution of the overlying air is much less than we are warranted in assuming. It seems also plain that we are not justified in forming anything more than a very rough conception of what is in progress, or what can occur under the conditions afforded by sewers and their contents. By the very nature of things these conditions undergo the widest range of variation. The composition of the sewage itself varies not only in different sewers, but in the same sewer at different times in the day; the dilution of the gaseous products of decomposition and fermentation, through the manifold portals of ventilation and diffusion, changes constantly with fluctuation in temperature, and particularly with the force and direction of the wind; the pollution and dilution of the air of the sewer through leakage into it of air from the surrounding soil is not the same for all geological formations through which the sewer may run, and it does not seem unlikely that some of the waste products that gain access to sewers, especially from various manufactories may play an important part in influencing the activity or character of the fermentations that are to occur. When viewed from these standpoints we must admit the impropriety of conceiving the air of sewers to be a specific compound, or one that is even likely to approach constancy in its composition.

When it long ago became evident that the potency of sewer air for the production of disease could hardly be explained by its chemical composition, attention turned to its biological characteristics, and it was assumed because sewage contained countless bacteria, and more or less frequently bacteria of a pathogenic nature, that the air over such sewage must also of necessity contain these organisms. Since we have learned that many of the diseases formerly believed to result from the respiration of the air of sewers are of a specific nature and depend for their existence upon the presence in the tissues of particular forms of micro-organisms, the opinion has been advanced that the part played by sewer air is that of a carrier of these infective particles from the sewage to the individual who has become infected. Have we any evidence in support of this opinion? None that is based upon reliable methods of investigation. Unfortunately we know little or nothing of the *nature* of the bacteria found in the air of sewers. The information that has been derived from studies made upon such air relates to the *number* of bacteria present, and this, as already stated, is demonstrated to be usually lower than that for the

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open streets and inhabited houses, and we have no trustworthy evidence that pathogenic bacteria have ever been detected in sewer air. Notwithstanding this, no less of an authority on sewage than Mr. Corfield,[22] as late as 1892, writes that, in addition to typhoid fever, proof has been adduced that diphtheria, erysipelas, pyæmia, septicæmia, and puerperal fever occasionally result from exposure to such air. If this be true, an agent possessed of such manifold potencies for the production of disease should certainly call for much more careful study by exact methods than it has thus far received. The teaching that, because materials containing organisms capable of producing disease are constantly gaining access to sewers, that the air of these sewers of necessity contains such organisms also, is simply an opinion ; it is not supported by observations that lead us to accept it as fact. With the demonstration by Naegeli[23] that bacteria do not arise from unmolested moist surfaces, or those in which agitation is moderate, though decomposition may be in active progress, and the confirmation of this observation by Pumpelly[24] and many others, the opinion was suggested that perhaps bubbling and splashing that may occur in putrefying solutions might be instrumental in wafting bacteria from such solutions into the superincumbent air. This opinion was very largely formulated from the results of experiments by Professor Frankland[25] who caused active bubbling to occur in solutions of lithium chloride by generating carbonic acid in them and testing the superincumbent air spectroscopically for lithium. By this method he was able to detect the presence of lithium in the air at the mouth of a conducting tube 21 feet long. He found also that the lithium particles passed through a layer of coarse charcoal five inches thick, placed in the tube. From these studies he concludes "that the breaking of minute gas bubbles on the surface of a liquid . . . . is a potent cause of the suspension of transportable liquid particles in the surrounding air ; and, therefore, when through the stagnation of sewage or constructive defects which allow of the retention of excrementitious matters for several days in the sewer, putrefaction sets in and causes the generation of gas, suspension of zymotic matters in the air of the sewer is extremely likely to occur."

The experiments of Pumpelly demonstrated that air aspirated through decomposing fluids at so low a velocity as $\frac{2}{3}$ of a litre per day always carried bacteria with it into another flask with which it was in communication.

Mr. A. E. Truby, student of bacteriology in our laboratory, has made a series of experiments under my direction on this point, the results of which, while in the main confirmatory of the experiments

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of preceding investigators, afford some additional light upon the subject. It was found, as others before had found, that with artificially produced bubbling, resulting from the aspiration of air through solutions containing bacteria, that a certain number of these are thrown into the superincumbent atmosphere, and some of them are carried in the direction of the main air current. The number of bacteria carried is dependent upon several factors, the principle being the quantity of air aspirated and the velocity at which it is drawn through the decomposing matters. He found that when from 5 to 7 litres of air were aspirated through solutions containing known species of bacteria, that none of these bacteria were transported into a flask of sterilized medium placed 1 meter distant at the end of a connecting tube of 15 millimeters inside diameter when the velocity of the air current in this tube did not exceed 8.6 cm. per minute, but that if the velocity was doubled, bacteria were carried over this distance in every case. On the other hand, in five experiments in which the length of the tube connecting the decomposiug infusion and the flask of sterilized medium was increased to 4.5 meters, 7 litres of air with the velocity increased *30 fold* were aspirated through without bacteria being carried this distance in three out of five cases (for details see Table VI). Do experiments of this nature in any way simulate conditions naturally found in sewers, and do results obtained from such experiments afford a trustworthy basis for the assumption that because of splashing and bubbling in the sewer, bacteria are of a necessity thrown into overlying air and conveyed through the manifold ramifying arms of the sewer into houses in connection with it? To me it seems most unlikely. In the experiments to which reference is made there is always a single direct current of air passing through the infected solutions, causing more rapid and vigorous bubbling than I have ever been able to detect, even in the most actively decomposing infusions, and passing thence for a longer or shorter distance through straight or slightly bent smooth tubes directly into a medium that is to demonstrate the presence of bacteria that the air may contain. There is *nothing* unfavorable to the success of such a demonstration when conducted within reasonable limits.

With the hope of approaching more closely to natural conditions I arranged a number of experiments in a different way, the object being to determine if in a closed chamber from which air currents were excluded bacteria did arise from substances undergoing bubbling as a result of decomposition. To this end a large bell jar of 40 centimeters diameter, 28 centimeters high and of approximately 37 litres capacity was employed. Under it was placed an open glass dish of

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about 11 centimeters in diameter and 7 centimeters high containing a fluid culture of bacteria, and arranged around this were Petri plates containing sterilized gelatine, from 4 to 7 having been used so that there was an exposure of from 310 to 550 square centimeters of nutrient gelatin upon which bacteria could fall from the air. Bubbling was brought about in two ways. First, spontaneously, by the addition to the culture of from 4 to 5 per cent. of glucose and then inoculating it with ordinary baker's yeast, and second, by the propulsion of air through it by means of a glass tube passed through an opening in the side of the bell jar. In both cases the bubbles arose to the surface of the fluid, burst and directed whatever was disengaged from the fluid straight up, the direction not being influenced by steady currents in one or another direction to any greater extent than could be avoided. Because of the manifold chances of contamination during the manipulation of so much bulky apparatus, this is hardly an ideal bacteriological experiment, but the results that have been obtained are, I think, of interest, for I believe they approach more nearly to what we may expect under conditions that are found in nature.

As a result we found that when a pure culture of an easily recognizable organism—the *bacillus prodigiosus*—was placed in a wide open vessel in the chamber and bubbling or foaming was induced through the action of yeast upon glucose, not a single colony of this organism appeared upon either of seven plates exposed in the chamber for *three* days. There did, however, appear a few colonies of other organisms, principally moulds, but similar results to this were obtained in a control experiment made in the same chamber from which the culture was absent, so that probably these colonies represented accidental contaminations.

When, on the other hand, bubbling was produced by propelling air through a similar fluid culture, at even so low a velocity as 3½ litres in 6 hours, the bubbles rising to the surface at the rate of 22 per minute, colonies of the organism were seen on the plates in varying numbers, from 12 to 38 per plate, on *the following morning*. As with the preceding experiment, a few foreign colonies were also present.

From these observations it is clear that the number of bacteria thrown into the air as a result of bubbling from spontaneous fermentation is probably very much smaller than results obtained from artificially produced bubbling would lead us to infer. Let us now compare these observations made in closed chambers under conditions within our control with the phenomena that are presumably occurring in sewers. In the first place, as ordinarily constructed, sewers do not occasion very much splashing of their contents except, perhaps, at

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isolated points. Secondly, there is no doubt that bubbling does occur, but it is probably feeble, at any rate I have never seen it to be active in sewage confined in vessels under temperature favorable to the process. Thirdly, the direction of air currents in sewers, as we know from the observations of Rosaheigyi and of Crimp (l. c.), is more often toward the mouth of the sewer than in a reverse direction, and almost as frequently there is no current at all. But let us suppose that a current did exist and always in the direction of the house, does it seem reasonable to suppose that this air, relatively poor in bacteria of all kinds, as experiment has shown it to be, would be likely to convey pathogenic bacteria, the presence of which is doubtful, from the sewer into the small branches, from these into still smaller branches, all moist, all presenting deviations from the straight line, and thence through soil pipes into houses, through water traps, or what is considered more dangerous, pin hole openings, the work of time or the careless plumber? The conditions that it would meet on its passage from the sewer into houses are not such as to favor an *increase* in the number of bacteria in it, for all experiments agree in demonstrating that such particles are not dislodged from moist surfaces even by winds of more than ordinary velocity. On the contrary, everything is favorable to a diminution in the number of organisms; with every change in the direction of the pipes conditions present that are favorable to the deposition of suspended bacteria upon the opposing moist surfaces, or their gravitation to the bottom, just as dust particles become deposited under similar circumstances. On this point the work of Carnelley and Haldane[16] demonstrated the interesting fact that in passing through a tube along the bottom of which water was flowing the air gradually lost a large proportion of its bacteria. As a mean of 7 experiments they found that in flowing as short a distance as 5 feet under these circumstances the number of bacteria in the air was reduced a little more than 50 per cent., and they conclude "that it seems almost unreasonable to suppose that micro-organisms can enter a house with the air of sewers, unless the draught is very strong and rapid." Carmichael[17] has also conclusively demonstrated that the intervention of water traps is a reliable safeguard against an invasion of bacteria, and that under ordinary circumstances they permit of less than 0·01 of a grain of gaseous matters to pass through them.

But why consume your time with arguments against the possibility of this air conveying bacteria from sewers into houses when there is nothing definite to lead us to believe that the bacteria of such air are pathogenic?

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If the limited studies that have been made upon the destiny of pathogenic bacteria, when in competition with the more vigorous saprophytic forms, are capable of general application, we may reasonably assume that the conditions existing in sewers are hardly favorable to the prolongation of the life of these organisms when deposited in sewers. Surely if this is not the case, or if they are not rapidly swept away with the current of sewage, which seems most improbable, we should observe the much more frequent occurrence of acute infections among the workers in sewers than is indicated by the results of statistical studies on the subject. To meet this latter point it has been suggested that the workers in sewers acquire a tolerance to the conditions by which they are surrounded; this is plausible in so far as the gaseous matters in the air are concerned, but I fancy no one would as yet feel justified in advancing this hypothesis with regard to the organized, infectious agents, if they were as numerous, and constantly present in the air of sewers as some would have us believe.

The more conservative of those who are inclined to the belief in a causal relation between the air of sewers and pathological conditions are gradually coming to the opinion that it is not directly concerned in the production of disease, but that its continuous respiration in some way interferes with the normal, vital resistance of the tissues and thus renders them more susceptible to infections to which they may be exposed. In very recent times observations have been made that may prove to have a most important bearing upon this opinion. The studies of Sanarelli[28] upon experimental typhoid fever resulted in the interesting demonstration that animals not normally susceptible to this disease could be rendered susceptible to inoculation with the bacillus of typhoid fever by previous injection into their tissues of the products of growth of several organisms not normally concerned in disease production, among these being the products of ordinary putrefactive bacteria. While it is true that such procedures do not simulate very closely the respiration of sewer air or air from other decomposing matters, still, they demonstrate that in concentrated form the products of decomposition have a decidedly depressing action upon the resistance of the animal into which they are introduced.

Of still more importance in their bearing upon the subject are the observations of Alessi[29]. He found that animals not normally open to infection by the *bacillus typhi abdominalis* became so after varying periods of exposure to the air of cesspools, and that the susceptibility was highest during the first two weeks of exposure, after which the

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animals seemed to gradually gain a tolerance and were then less susceptible to infection.

I have also endeavored to get some light on the subject by experiments on animals but my work on this line was begun so late that I am hardly prepared to do more than mention it.

The experiments were arranged with the idea of determining if prolonged respiration of the gaseous products of decomposition would have any effect upon the health of animals, or their resistance to infection. With this in view 4 animals (3 guinea pigs and 1 rabbit), were confined in glass chambers through which was aspirated air that had bubbled through decomposing infusions of meat; as this was the only air obtainable by the animals for respiration, and as the infusions through which it bubbled were advanced in putrefaction, it is fair to assume that the volatile products of decomposition reached the animals in a tolerably concentrated form. Of the three guinea pigs thus confined one (Pig A), was inoculated subcutaneously with fluid cultures of the bacillus typhi abdominalis twice, and subsequently into the peritoneal cavity with a similar culture twice. The first inoculation was subcutaneous and was made after the animal had been respiring the gaseous products of decomposition for 14 days; the last was into the peritoneal cavity and was made 6 days before death. The animal lived 30 days from the beginning of the experiment, having been confined in the chamber for 16 days of this period. It diminished in weight during this time from 460 to 300 grams, and had at one period a sudden fall in temperature to 35° C., immediately following inoculation. During the balance of the time the temperature fluctuations were unimportant.

At autopsy on this animal there was no evidence of an acute or general infection, bacteriological examination revealing the presence of typhoid bacilli in only the sites of subcutaneous inoculation, the liver and bile. The most important tissue changes were those presented by the kidneys. They were strikingly altered in their naked eye appearance, the surface was roughened by elevations and depressions, but the capsules was not adherent and stripped off clean and easily. The color was not markedly altered.

Sections of these organs, hardened in alcohol, present features corresponding with the gross appearances. The areas of depression on the surface correspond to foci of proliferating fixed connective tissue elements. A considerable number of small granulation cells and larger fibroblasts occupy these areas, obscuring many of the tubules and compressing others. The glomeruli were likewise surrounded by such cells and in addition, the capsule of Bowman was at many points much

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thickened. These areas, which were usually triangular in shape, with the bases at the capsule, were much contracted, the partially atrophied glomeruli being, by this means brought closer together. The thickening seen in the capsule of Bowman is due to the presence of an excess of fibrous tissue, poor in cellular elements. Adjoining the atrophic areas the tubules were seen to be dilated and the epithelium lining then was swollen and degenerated. In a few tubules hyaline casts were observed. In short, the condition was identical to that seen in the kidneys of human beings dead from chronic Bright's disease. (See photograph No. 1 and No. 2.)

The next animal, Rabbit V, was confined in the same chamber and kept under similar respiratory conditions. It lived 21 days, during which interval fluid cultures or suspensions of the *bacillus typhi abdominalis* were injected into its circulation by way of the ear vein on three occasions, 1.5 c.c. having been used for each injection. The animal died on the day following the last injection as did a control animal not exposed to the gases of decomposition.

During the 21 days that it was under observation its weight fell from 1080 to 995 grams and its temperature fluctuations were insignificant. At autopsy typhoid bacilli were recovered from the liver, and spleen ; the blood, urine and kidneys being negative. As with pig A the conspicuous alterations in this animal were those found in the kidneys. They presented upon the cortical surface a remarkable mottled appearance, being thickly studded with round, pale, flat areas that looked to the naked eye like minute cysts, about the size of a pin point. A number of small red points were also seen. The capsule stripped off easily and clean. On section the cortical portion is red, the medullary pale. Sections of these organs, hardened in absolute alcohol, revealed conditions suggestive of the action of a very energetic poison. Unlike the chronic conditions found in pig A, there was here an acute process. Everywhere throughout the entire organ the intertubular capillaries were literally plugged with polynuclear leucocytes, causing them to stand out in bold contrast with the tubular elements, particularly the convoluted tubules. The epithelium of the convoluted tubules was granular and necrotic, the nuclei did not take up the staining and in a few of these tubules fibrin was demonstrated by the method of Weigert. It is interesting to note that this necrosis was especially seen in the epithelium of the convoluted tubules and did not extend to that of the collecting tubes, the epithelium of which showed but little deviation from the normal. At many points of contact this difference was conspicuous. The nuclei of both parenchymatous and interstitial elements frequently gave evidence of the action upon it of

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a violent poison, by destructive changes of varying degree that were present at different points. The glomerular nuclei were conspicuously affected, being fragmented, distorted, pinched off into buds or drawn out into long thread-like processes and otherwise altered in shape. Many of the glomerular capillaries were hyaline and at some points there was an accumulation of granular matter within the capsule of Bowman. The entire process was one that suggested the activity of a violent toxic agent. (See photograph No. 3.)

Control animals inoculated in different ways with cultures of the typhoid bacillus showed at autopsy no appreciable alterations in the kidneys.

Two guinea pigs were then placed in the chamber and subjected to the gaseous products of decomposition alone; not being inoculated at all.

The experiment was begun April 11th, 1894 and continued with no interruption for 27 days, when, on the night of the last day the aspirator ceased to work and both animals were asphyxiated during the night.

At autopsy there was no macroscopic evidence of decomposition, nor did sections of the organs show such change. There was nothing abnormal about the kidneys of either animal to the naked eye.

They were placed in absolute alcohol and in Fleming's solution. In addition to the changes that will be described from the alcohol preparations those from Fleming's solutions showed the presence of a few minute black points, doubtless fat, in the epithelium of the convoluted tubules.

Further examination of sections from the kidneys of both animals showed a striking increase in the number of nuclei apparently between the tubules and surrounding the glomeruli in the cortex. The increase of nuclei was not equally present in all parts, nor was it present to the same degree in both animals.

In pig K, in which the change was less marked, the increase was apparently limited to the blood vessels. Large areas of the sections often showed no deviation from the normal, whereas along the distribution of one or two intertubular arteries the increase of nuclei was apparent. For the most part the cells presented the appearance of fibroblasts, were possessed of epitheloid nuclei and in no instance could fibres be made out between them. Sometimes the increase was especially about the glomeruli, resulting in an apparent thickening of the capsule, at other times it was more marked between the tubules.

In the case of pig J (the companion of the preceding animal), the changes resulting from cellular proliferation in the cortex are still more marked. (See photograph No. 4.) In the first place the areas of cellu-

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lar proliferation are much more numerous and the distribution is somewhat different. Such localizations as have just been described in the companion animal (pig K) namely: the increase of cells along the cortical vessels, is found here also, but in addition there are small groups of cells just beneath the capsule, extending a short distance downwards into the substance of the kidney, and associated at these points with a slight indentation of the surface of the organ. In this animal, as little as in the other, could fibres be made out distinctly between the cells. The Fleming preparations showed, as did those of the other animal, a small amount of fat in the epithelium of the convoluted tubules. Beyond this it cannot be said that marked changes in the epithelium were demonstrable. A control guinea pig of the same lot, killed for purposes of comparison, showed in the kidney lesions that are similar to those described in the last two animals, although they are less marked than in the latter (pig J) and more marked than in the former (pig K). In fact they correspond so closely in many respects with the lesions seen in pig K that we feel further investigations upon new lots of animals to be necessary before definite conclusions can be drawn. If, however, one is justified in formulating an opinion from the results that have been obtained in these experiments, we think it evident that they suggest the existence in the concentrated emanations from decomposing matters of substances which when respired, have the power of producing alterations in the integral elements of the kidneys of animals, but we do not, as yet, consider this point demonstrated.

It is equally interesting that animals can live for so long a time in such an atmosphere and experience no greater pathological lesions than those to which allusion was made in guinea pigs J and K. If the results of experiments made upon animals in this way are of any value in demonstrating the positive or negative effect of air saturated with gaseous products of decomposition, it does not seem reasonable from these studies to suppose that the air of a sewer or cesspool, in the enormous dilutions that it exists at the time it reaches an individual in a house to which it has access, can be of much importance in either the direct production of diseased conditions or, in alone influencing the vital powers of resistance of the individual who inhales it.

NOTE.—Before closing I wish to acknowledge my indebtedness to Dr. Simon Flexner, Associate in Pathology, Johns Hopkins University, for his valuable assistance in the study of the pathological conditions found in the animals upon which the above experiments were performed.

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PHOTOGRAPHS.

No. 1—Section through kidney of Guinea Pig A; showing condition of chronic interstitial nephritis. × 125.

No. 2—Section through another portion of same kidney. × 60.

No. 3—Section through kidney of Rabbit V; showing accumulation of polynuclear leucocytes in the intertubular capillaries and necrotic condition of tubular epithelium. × 125.

No. 4—Section through kidney of Guinea Pig J; showing accumulation of fibroblasts at points about the glomeruli; and in some places apparent thickening of the capsule of Bowman. × 125.

NOTE.—I am under many obligations to Doctor Walter Reed, Surgeon U. S. Army, Curator of the Army Medical Museum, Washington, D. C., for his kindness in having had these photographs made for me.

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TABLE I.

Showing condition of the air over decomposing substances confined in closed vessels, after varying intervals. All maintained at the ordinary temperature of the laboratory.

Date.	Material.	Period of confinement in flask.	Oxygen, as vol. per cent.	Total gases absorbed by KOH. Vol. per. cent.	Remarks.
Oct. 5, 1893.	Sewage.	12 days.	17.25	1.42	No distinct odor, neutral reaction.
Nov. 8, 1893.	"	35 "	13.39	4.12	" " " " "
Feb. 12, 1894.	"	35 "	6.50	14.4	Consists of slime, etc., scraped from the bottom of sewers and mixed with fluid sewage.
April 23, 1894.	"	7 "	0.45	23.63	Consists of slime, etc., scraped from the bottom of sewers and mixed with fluid sewage.
Oct. 1, 1893.	Meat infusion.	8 days.	0.27	34.8	These infusions consisted of about 50 grams of finely chopped lean meat to a litre of water. All samples, including the sewage samples, were kept in hermetically sealed flasks and were only opened for analysis. They were all maintained at the ordinary temperature of the laboratory.
Nov. 4, 1893.	"	15 "	1.54	19.28	
April 23, 1894.	"	7 "	0.22	29.38	

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TABLE II.

Analyses of air passing continuously over decomposing infusion of meat confined in flask.

Date.	Litres of air drawn through during preceding day	Rate of renewal of air in flask for preceding day.	Total CO_2 expressed as milligrams.	Total H_2S as milligrams.	Relative CO_2 as milligrams per litre.	Relative H_2S as milligrams per litre.	Temperature fluctuations in degrees C.	
Oct. '98								
23	58.3	19.4	82.2		1.4		23	
24	58.3	19.4	82.2		1.4		20	
25	69.6	23.2	156.0		2.24		20.5	
26	72.0	24.0	182.5		2.53		16	
27	90.0	30.0	376.2	35.8	4.18	0.103	21	
28	69.6	23.2	183.0		2.63			
29	90.5	30.2	185.3		2.05			
30	90.5	30.2	185.3		2.05			
31	----	----	----		----	----		
Nov.								
1	186.5	62.2	326.8	12.6	1.75	0.029		
2	193.9	64.6	423.4	16.2	2.18	0.084	26	
3	110.6	36.9	602.8	11.0	5.45	0.099	21	
4	148.8	49.6	491.2	6.1	3.30	0.041	17.5	
5	148.9	38.3	465.6	2.4	3.13	0.016	13	Note preceding fall of temperature. Observe its effect on H_2S. Compare with chart.
6	66.5	22.2	176.7	69.0	2.66	1.038	24	
7	74.1	24.7	526.2	0.00	7.11	0.00	23	
8	4.26	1.42	279.1	1.1	65.5	0.258	20	
9	5.16	1.72	398.5	0.3	77.2	0.058	22	
10	4.08	1.36	312.8	0.00	76.7	0.00	21.5	
11	6.10	2.03	374.2	0.00	61.4	0.00	23	Note effect of fall in temp. upon H_2S. Compare with chart.
12	4.30	1.43	263.0	8.7	61.2	2.023	10	
13	5.54	1.85	244.1	4.7	44.1	0.848	15.5	
14	5.41	1.80	251.5	0.4	46.5	0.074	24	
15	58.0	19.3	449.7	0.00	7.8	0.00	16	Fall in temp. Note effect on H_2S. Compare with chart.
16	53.6	17.9	145.6	0.1	2.72	0.002	8	
17	101.5	33.8	270.5	0.8	2.67	0.008	25	
18	87.	29.0	214.5	3.60	2.47	0.042	26	
19	147.	49.0	246.5	5.1	1.68	0.035	22	
20	73.8	24.6	152.7	0.00	2.07	0.00	24	
21	55.6	18.5	237.3	5.5	4.27	0.099	21	
22	128.0	43.0	275.9	7.1	2.16	0.06	25	
23	129.4	43.1	236.2	13.2	1.83	0.102	26	
24								
25	α=33	11	233.0	1.4	----	----	25	Flask sealed since 23d.
	β=9	9	113.0	4.5	----	----	25	
Dec.								
2	α 9	3	425.	1.8	----	----	22	Flask sealed since Nov. 25. Note effect of reduction in temp. on H_2S.
	β 9	3	117.	5.0	----	----	15	

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Table III.

Analyses of air passing continuously over sewage confined in flask.

Date.	Litres of air drawn through during preceding day.	Rate of renewal of air in flask for preceding day.	Total CO_2 expressed as milligrams.	Total H_2S expressed as milligrams.	Relative CO_2 as milligrams per litre.	Relative H_2S as milligrams per litre.	Temperature fluctuations in degrees C.	
Dec'93.								
11	91.6	30.5	108.	2.3	1.2	0.03	22	
12	84.8	28.3	92.4	2.2	1.1	0.03	24	
13	89.5	29.5	123.2	7.5	1.4	0.084	21	After analysis of 14th flask was gently agitated and dense pellicle that had formed on the surface of the sewage was ruptured. Note effect on H_2S. See chart.
14	82.0	27.3	94.7	0.00	1.2	0.00	20	
15	124.5	41.5	114.0	61.5	0.92	0.494	24	
16	110.5	36.8	103.7	12.9	0.94	0.120	26	
17	242.0	80.6	106.2	28.7	0.44	0.12	19	
18	167.9	55.9	57.6	12.9	0.34	0.08	18	
19	----	----	----	----	----	----	----	
20	177.7	59.2	99.3	3.1	0.56	0.017	21.5	
21	168.	54.3	58.2	0.00	0.36	0.00	22	
22	----	----	----	----	----	----	----	
23	80.5	26.8	105.5	3.8	1.31	0.48	21	
24	129.9	43.3	83.4	16.3	0.642	0.13	23	
25	73.9	24.6	51.4	16.7	0.70	0.21	20	Note effect of sudden fall in temp. in causing increase of H_2S. See chart.
26	145.0	48.3	135.5	73.5	0.94	0.50	12	
27	123.0	41.0	44.3	18.6	0.36	0.15	20	
28	245.3	81.8	----	----	----	----	20	
29	37.5	12.5	26.0	21.2	0.61	0.57	23	
30	170.8	56.9	53.0	71.5	0.31	0.42	18	

Sample 2, from another sewer.

Jan. '94								
10	86.5	28.8	354.9	1.3	4.10	0.015	21.5	
11	98.5	32.8	245.4	0.7	2.50	0.007	19	
12	107.3	35.8	209.7	0.00	2.00	0.00	15	
13	185.2	61.7	242.8	0.00	1.31	0.00	18	
14	120.	40.	84.4	0.6	0.70	0.005	19–14	Sudden fall in temp. Note effect on H_2S.
15	58.2	19.4	35.4	1.9	0.61	0.033	22	
16	65.	22.	77.5	43.3	1.21	0.67	24	After weighing on 15th, a dense pellicle that had formed on the surface of the sewage was broken. Note effect on H_2S. See chart.
17	181.6	60.5	136.4	38.8	0.751	0.22	23	
18	181.2	60.4	81.1	59.0	0.48	0.33	22.5	
19	156.3	52.1	116.4	40.6	0.75	0.26	24	

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TABLE IV.

Fractional distillations at 100° C., to determine the rate at which free ammonia and substitution products of ammonia pass off from solutions containing them.

	Material tested.		Amount.	Amount of water employed.	Distillation with or without pumice stone.	Contents of each fraction of 50 c.c. of distillate.								Total NH_3 found.	Correction for NH_3 in water employed.	Correct'd total.
						1st	2d	3d	4th	5th	6th	7th	8th			
A	Ammonium chloride (NH_4Cl)	I	=1 mg. NH_3	500 c.c.	With	0.900	0.175	0.033	0.010	0.002	0.000			1.120	0.0185	1.1015
		II				0.900	0.175	0.040	0.007	0.0025	0.000			1.1245		1.106
B	"	I	=5 mgs. NH_3	750 c.c.	With	3.250	1.300	0.475	0.170	0.055	0.018	0.006	0.002	5.2810	0.027	5.254
		II				3.250	1.200	0.5250	0.170	0.055	0.014	0.0055	0.000	5.2195		5.1925
C	"	I	=1 mg. NH_3	750 c.c.	With	0.750	0.250	0.090	0.033	0.0105	0.0025	0.000	0.000	1.141	0.0425	1.0985
		II				0.800	0.1900	0.090	0.033	0.0105	0.0025	0.000	0.000	1.151		1.0885
D	Ammonium chloride (NH_4Cl)	I	=5 mgs. NH_3	500 c.c.	Without	3.250	1.200	0.3125	0.075	0.024	0.006	0.0015	0.000	5.069	0.0295	5.0395
		II				3.500	1.150	0.300	0.060	0.013	0.006	0.000	0.000	5.028		4.9985
E	"	I	=1 mg. NH_3	500 c.c.	Without	0.830	0.220	0.065	0.019	0.0085	0.003			1.1405	0.0095	1.131
		II				0.8300	0.1900	0.045	0.013	0.0025	0.000			1.0805		1.071
F	"	I	=5 mgs. NH_3	750 c.c.	Without	2.750	1.200	0.550	0.2438	0.0875	0.045	0.016	0.0045	4.8968	0.0145	4.8823
		II				2.900	1.100	0.575	0.2375	0.0563	0.0365	0.011	0.0035	4.9198		4.9053
G	"	I	=1 mg. NH_3	750 c.c.	Without	0.600	0.325	0.125	0.045	0.014	0.005	0.0025	0.002	1.1185	0.0155	1.103
		II				0.650	0.275	0.125	0.055	0.0225	0.012	0.0025	0.0015	1.1435		1.128
H.	Trimethyl-amine.		5 c.c. of about 1 per cent. sol.	500 c.c.	With	1.150	0.2875	0.085	0.017	0.003	0.000			1.5425		
	Methylamine.		5 c.c. of about 1 per cent. sol.	500 c.c.	With	1.050	0.240	0.070	0.014	0.000	0.000			1.3740		
	Methylamine.		20 c.c. of about 1 per cent. sol.	500 c.c.	With	3.500	1.050	0.2375	0.0650	0.0140	0.0035			4.870		
	Trimethyl-amine.		20 c.c. of about 1 per cent. sol.	500 c.c.	With	4.8260	1.300	0.2933	0.055	0.0095	0.000			6.5038		
I	Mixture of Methylamine. Trimethyl-amine and Ammonium chloride		5 c.c. of 1 per cent. sol. 5 c.c. of 1 per cent. sol. $2\frac{1}{2}$ mgs. NH_3	500 c.c.	With	4.400	1.050	0.2850	0.070	0.013	0.0045			5.8225		

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TABLE V.

ESTIMATION OF FREE AND ALBUMINOID AMMONIA IN AIR FROM DIFFERENT SOURCES. RESULTS EXPRESSED AS FRACTIONS OF A MILLIGRAM.

(1) Date.	(2) Source of air analyzed.	(3) Amount of air taken, in litres.	(4) Time consumed (in hours) in aspirating.	(5) Total ammonia found.		(6) Ammonia in water used. To be deducted.		(7) Amount of ammonia in air analyzed.		Remarks.
				Free.	Alb.	Free.	Alb.	Free.	Alb.	
Dec. 23, 1893	Outside air.	129.9	24	0.018	0.0025	0.0145	0.0025	0.0035	0.000	The air obtained for these analyses came from just outside a window of the laboratory, on the second floor, about 35 or 40 feet from the ground.
Dec. 23, 1893	" "	35.2	25.5	0.0155	0.0025	0.0145	0.0025	0.001	0.000	
Dec. 24, 1893	" "	230.8	49	0.0145	0.0025	0.0145	0.0025	0.000	0.000	
Dec. 24, 1893	" "	47.6	50.5	0.0145	0.0025	0.0145	0.0025	0.000	0.000	
Dec. 26, 1893	" "	355.4	48	0.0095	0.0018	0.006	0.0018	0.0035	0.000	
Dec. 26, 1893	" "	108	46.5	0.0075	0.0025	0.005	0.0025	0.0025	0.000	
Dec. 28, 1893	" "	56.1	20.5	0.0075	0.0025	0.005	0.0025	0.0025	0.000	
Dec. 28, 1893	" "	182.2	25.5	0.0073	0.0025	0.005	0.0025	0.0023	0.000	
Dec. 18, 1893	Air of laboratory	31.4	2.5	0.009	0.009	0.009	0.009	0.000	0.000	The air used for these analyses was from my own room in the laboratory.
Dec. 21, 1893	" "	105.3	22	0.0155	0.003	0.0145	0.003	0.001	0.000	
Dec. 22, 1893	" "	76.3	4.5	0.0175	0.003	0.0145	0.003	0.003	0.000	
Dec. 29, 1893	" "	169.5	22.5	0.008	0.0025	0.008	0.0025	0.000	0.000	
Dec. 29, 1893	" "	207.1	21.5	0.009	0.002	0.0085	0.002	0.0005	0.000	
Dec. 30, 1893	Air from sewage.	36.9	5	0.036	0.0025	0.008	0.0025	0.028	0.000	In these experiments the air was aspirated over sewage contained in a flask. About four litres of sewage was placed in an eight litre flask for the experiments.
Dec. 30, 1893	" "	141.2	5	0.008	0.004	0.0085	0.002	0.000	0.002	
Jan. 3, 1894	" "	36.1	4	0.072	0.0065	0.008	0.0025	0.064	0.004	
Jan. 3, 1894	" "	108	4.5	0.010	0.004	0.008	0.0025	0.002	0.0015	
Jan. 18, 1894	" "	254.7	42.5	0.042	0.0023	0.0165	0.0023	0.0255	0.000	

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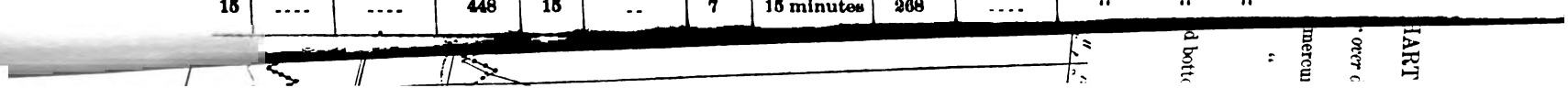
TABLE VI.

Transportation of bacteria by air bubbling through decomposing solutions.

	Length of tubes in centimeters.		Distance from culture to sterilized medium in cm.	Calibre of tubes in millimeters.		Litres of air aspirated.	Time consumed in aspirating.	Velocities of air current in tubes, in cm. per min.		Remarks Results
	Main.	Connections.		Main.	Connections.			Main.	Connections.	
1	90	40	130	15	5	7	45 minutes	88	798	Bacteria carried over, 6 colonies
2	60	40	100	15	5	7	45 minutes	88	798	" " " 5 "
3	140	90	280	15	5	7	90 minutes	44	397	" " "
4	200	40	240	15	5	7	90 minutes	44	397	" " "
5	90	40	130	15	5	7	120 minutes	33	297	" " "
6	60	40	100	15	5	7	240 minutes	16.5	149	" " "
7	140	90	280	15	5	5½	360 minutes	8.6	78	No bacteria carried over.
8	90	40	130	15	5	7	480 minutes	8.3	74	" " " "
9	90	40	130	15	5	4	360 minutes	8.0	57	" " " "
10	60	40	100	15	5	3½	420 minutes	4.8	42.5	" " " "
11	....	....	448	15	..	7	30 minutes	184	....	" " " "
12	....	....	448	15	..	7	15 minutes	269	..	" " " "
13	....	....	448	15	..	7	15 minutes	268	.. .	" " " "
14	....	....	448	15	..	7	15 minutes	268	....	Bacteria carried over.
15	....	....	448	15	..	7	15 minutes	268	....	" " "



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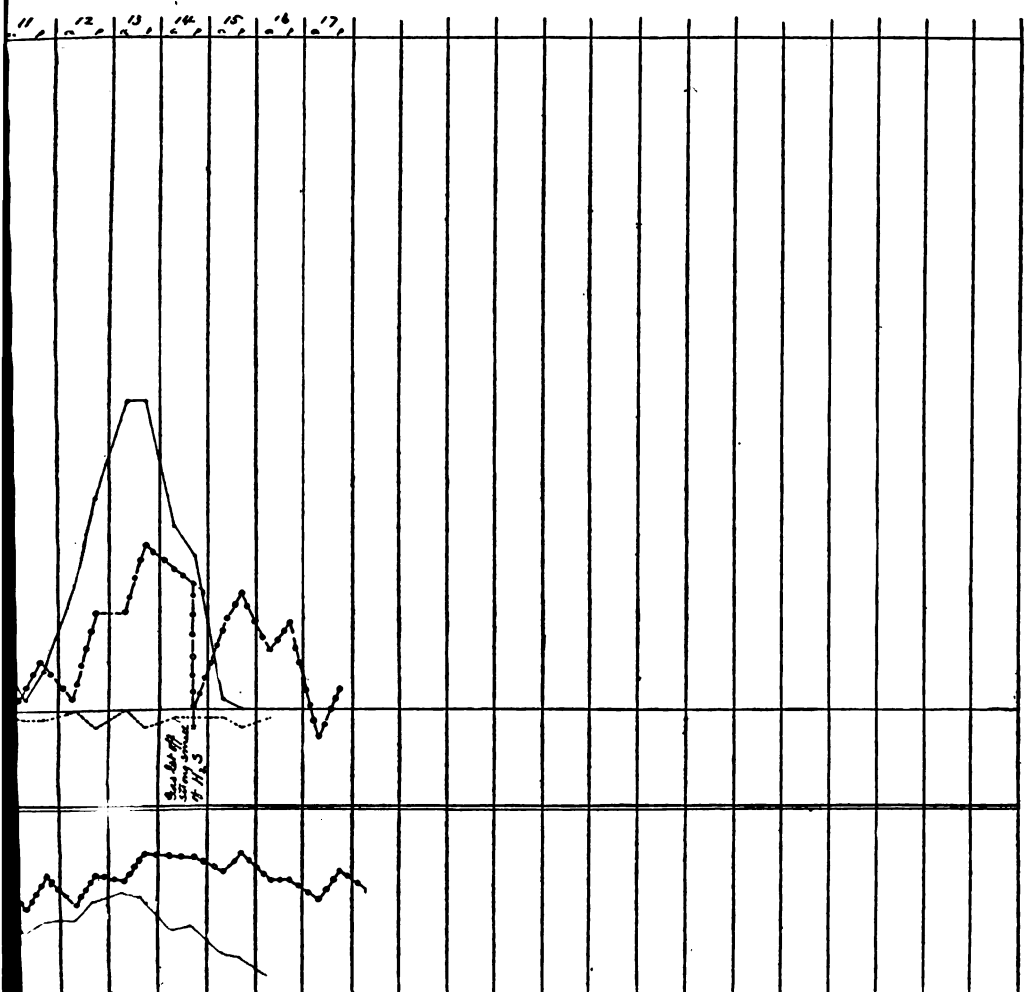
HART I

- *over decomposing infusion of meat. All kept under same conditions.*

mercury. Infusion confined in hermetically sealed flask. Experiment of Sept. 22nd, 1893.

" Sewage " " " " " Experiment of Sept. 26th, 1893.

d bottom of sewer. Mixture confined in hermetically sealed flask.



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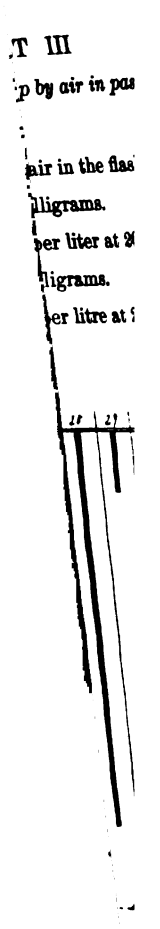

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RT II

rogen taken up by air in passing over an actively decomposing infusion of meat.

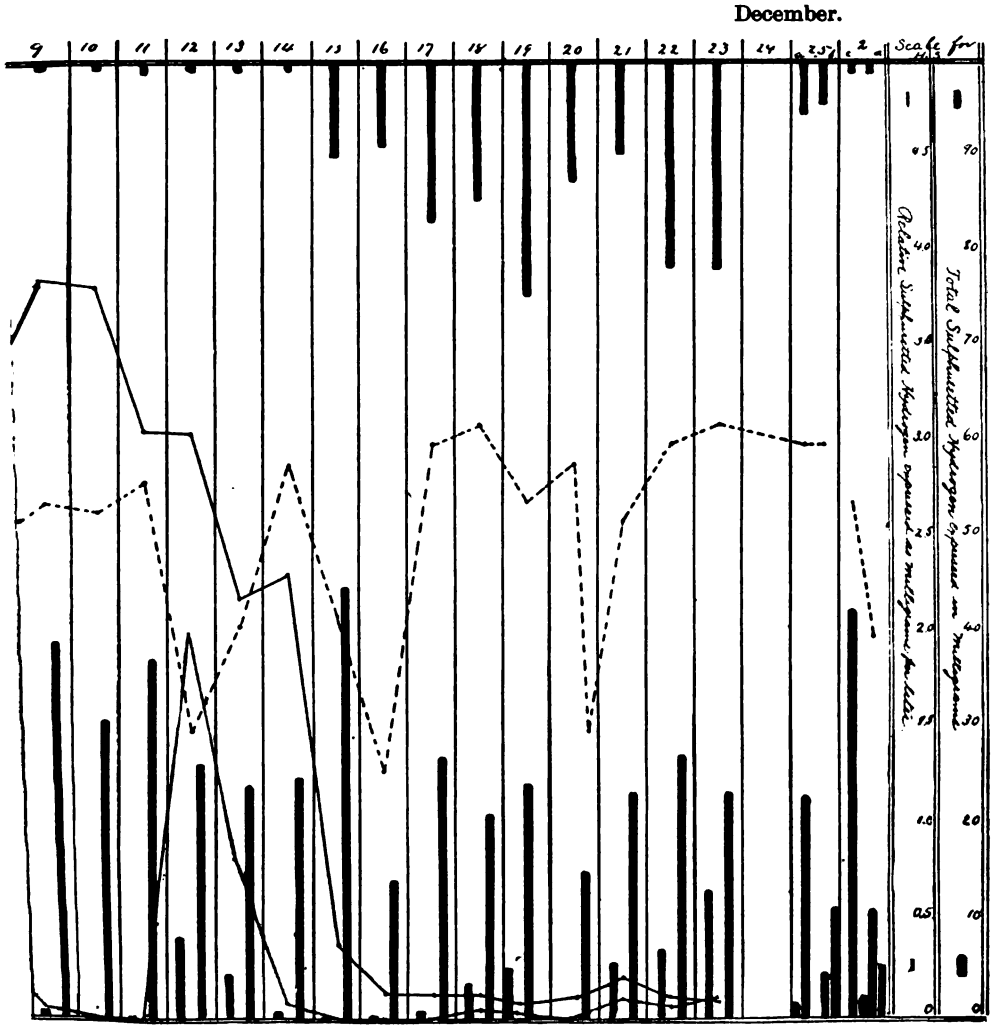
ir in the flask (3 liters) was renewed during the preceding 24 hours.

igrams.

r liter at 20° C. being equivalent to about 10.7 parts per 10,000 of air (per volume).

igrams.

r liter at 20° C. being equivalent to about 7.84 parts per 10,000 of air (per volume).



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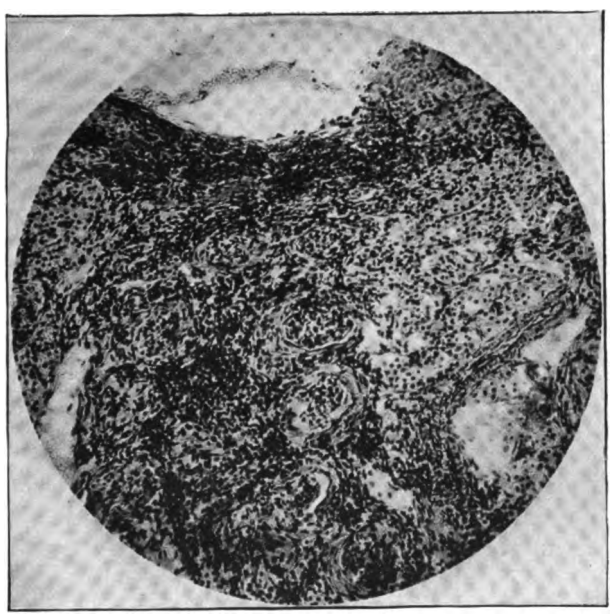

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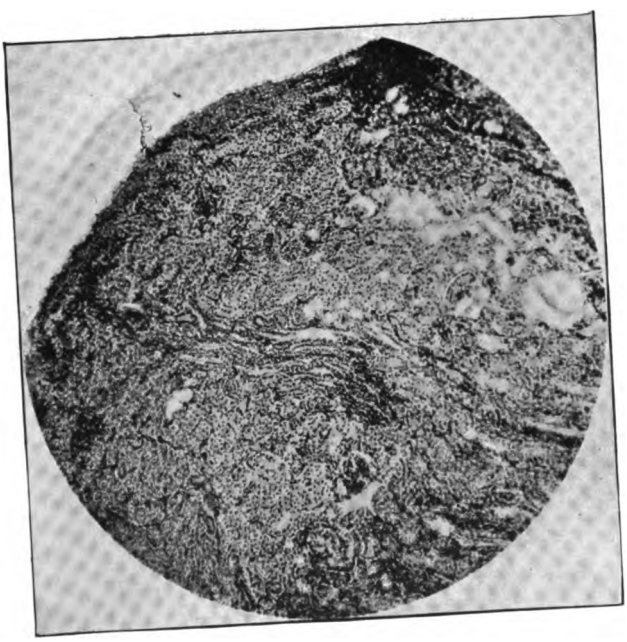

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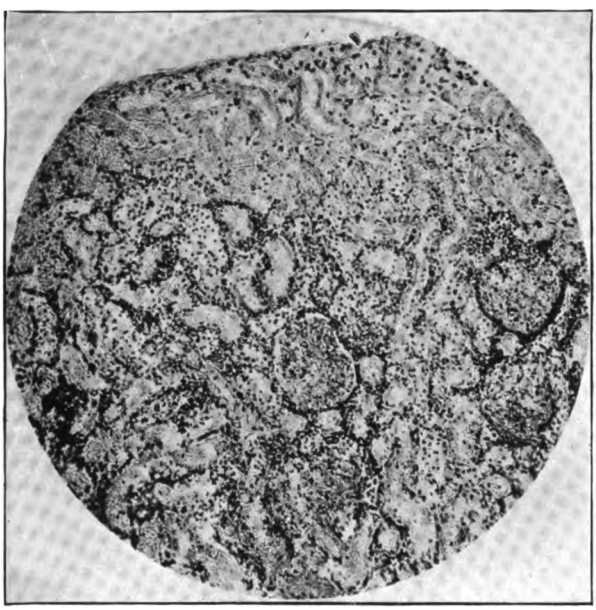

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THE PRODUCTION OF DISEASES BY SEWER AIR.

BY A. JACOBI, M.D.,

Clinical Professor in the College of Physicians and Surgeons, New York.

THE composition of sewer air is at least as variable as that of sewerage. According to Henry R. Kenwood (*Public Health Laboratory Work*, Philadelphia, 1893, p. 232) its reaction is generally alkaline. Oxygen is variously diminished, according to the efficiency of the sewer ventilation; it is sometimes in normal proportions. Carbonic acid is variously increased from the same cause; it probably does not average more than twice the normal amount. Ammonium, sulphureted hydrogen, ammonium sulphide, and carbon bisulphide are present in small quantities. Marsh gas is small in amount or absent. The fœtid and putrid organic vapors of sewage are, according to Odling, allied to the compound ammonias, and are probably carbo-ammoniacal, and contain traces of ptomaines and leucomaines (*i. e.*, animal alkaloids). Moulds, fungi, and bacteria (chiefly bacilli) and their spores, together with animal and vegetable *débris*, appear to constitute almost the entire suspended matter. Micro-organisms average about six per litre in the air of a good sewerage system.

The atmospheric air always contains bacteria, mostly, it is true, dead, and mineral parts. The presence of pathogenic germs has been denied; but there *must* be some in the air, and living ones too, for contagion, unless it result from immediate physical contact of the sick and the well, must take place through the air. Tubercle bacilli are found on the walls of rooms; before they enter the lungs of inmates, they must be carried through the air with other dust. It is true, they have been found there but rarely; but von Eiselsberg claims to have seen *Streptococcus erysipelatos* (*Langenbeck Archiv*, vol. xxxv, 1886) and Pawlowsky *Pneumococcus Friedlander* (*Berl. klin Woch.*, No. 22, 1885). Indeed, the general statement of Tyndall, not contradicted, always reaffirmed (*Essays on Floating Matter of the Air*, New York, 1882), that the apparently purest air contains dust with micro-organisms, makes the frequent presence of pathogenic organisms at least probable.

But the atmosphere is certainly no favorable medium. Germs are heavy and fall to the ground; thus it is not unreasonable to believe, but it can not be proved, that a walking child of two feet in height may inhale them more readily than an adult whose respiratory inlet is

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more than five above the surface. The dryness and light of the sun destroy them; even micrococci die in sunlight in a few hours (Duclaux, *Microbes et maladies*, p. 34). It is only when locked up that spores were found normal after many (twenty-five) years. Koch retained virulence in his tubercle bacilli five or seven days in diffused light, but only a few minutes or at most hours under sun rays.*

Indeed, pathogenic bacteria have a hard time of it. They live in high temperatures only, and die soon in a low one; they are readily destroyed in water containing saprophytes or any other non-pathogenic bacteria. In the thoroughly soiled water of the River Seine, at Paris, which holds no oxygen, there are no pathogenic bacteria; while a few miles farther down, near Meudon, the Seine contains again both oxygen and pathogenic bacteria. Hence, sewage is not a promising place for them to thrive or live in. Great dilution destroys them or renders them innocuous. For two thousand years Rome has emptied all its fæces and other refuse into the Tiber, and no impurities of a dangerous character were detected by Celli and Scala a few miles below the city.

Now, what is valid for air outside a sewer is so for that inside it, with this difference, that there are more germs found in the atmosphere than in sewer air. Billings states emphatically that there are fewer micro-organisms in the air of sewers than in that of the streets. He quotes Carnelly and Haldane (*Proceedings of the Royal Society*, London, 1847, p. 51), who report that the London and Dundee sewers contain twice as much carbonic acid, three times as much organic matter as outside air, and fewer micro-organisms, and remarks that this air in the sewers is better than in naturally or even mechanically ventilated schools. It is only when there is splashing in the sewers that (temporarily) there can be more organisms in their air. Otherwise moist surfaces do not give them off. It is only under favorable circumstances that they can be carried off and upward into the houses and escape through ventilating shafts. Into living rooms they could escape only either where there are no traps, or where the traps are empty either from disuse or from being sucked out, or from upward pressure. In this way, Billings suggests, pyogenic organisms and Fehleisen's coccus appear to be conveyed through house drains.† At

* The statements occasionally made that *Achorion Schönleini*, plasmodia, erysipelas cocci, also tubercle and typhoid bacilli, and vibrio choleræ may undergo multiplication in the air, lacks confirmation.

† The epidemic of enteric fever in Croydon, 1875, was attributed by Buchanan to the entry of infected sewer air into houses through untrapped drains and openings into the drains. The pipe sewers were of small size, six or nine inches in diameter, and were ventilated at distances of a hundred and fifty to two hundred and fifty yards by petty openings which were blocked by charcoal trays.

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all events the opportunity for microbes to get out of the dwellings is greater than to get into them. When they get into the drains from inside, they are flushed out. It is evident, however, that the flushing out of substances entering the sinks from inside depends on the structure and size of the drain, the nature of the trap, and the amount of water poured through it, also on the use or non-use of disinfectants employed in the households.*

Less rainfall, and consequently less flushing of sewers, gives rise to accumulation of more filth. Badly constructed brick sewers have the same result. Outfall sewers terminating below water are apt to be choked. Thus, while Russell's analysis yielded a fair standard of purity of sewer air, Parent Duchatelet found only 13·79 per cent. of oxygen and 2·99 per cent. of sulphureted hydrogen. Thus gas is undoubtedly developed to a great extent, bubbles are constantly breaking on the surface (Frankland on The Transport of Solid and Liquid Particles in Sewer Gases, *Proceedings of the Royal Society*, April, 1877), and may enter houses through *untrapped* drains *whenever they are not permitted to leave the main sewer otherwise.*

Under the circumstances, as the specific germs of infectious diseases may be contained in the liquid disseminated by the bursting of bubbles, sewer air may certainly become specifically infected. Some of the germs may find a favorable medium in the organic material, the ammonia and the phosphates of sewage, while others are more liable to be destroyed by the saprophytes of putrefaction. As to typhoid, the cases are very numerous. In regard to cholera Parkes refers to its introduction into Southampton in 1866, where it was probably due, in his opinion, to the passing of pumped sewage, infected with cholera evacuations, in a frothy and agitated condition along an open conduit. He adds the remark that, as soon as the latter was covered over, the epidemic (or rather endemic) abated. The latter remark is suggestive. A sewer disconnected from houses by good traps is no longer an open conduit; and it appears that *unless sewer air is forced upward*, no amount of cholera bacilli or toxin will annoy the population of houses

* The quantity of refuse from rooms and houses is very large indeed. Pettenkofer calculates the daily amount of fæces for the average person at 90 grammes, of urine 1,170 grammes; for a thousand persons per annum, fæces, 34,000 kilogrammes; urine, 428,000 litres. If you add to that figure 159 litres of water daily for each individual, the sum total of daily sewage for a thousand persons is 160,000 litres. That explains in part the wrath of Andrew Fergus, M.D. In the *Proceedings of the Medico-Chirurgical Society of Glasgow*, of October 2, 1868, he broadly states that water-closets and canalization are opposed to revelation and Bible, that they are contrary to Nature, inasmuch as they rob the soil, are the sole cause of pollution of rivers, and fill the sewers with noxious gases which enter the houses in spite of traps.

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properly secured by traps and by ventilating shafts both in the houses and in the streets.

But granted that sewers are infested with bacteria, how do they get into the air of sewers, of streets, of houses?

Mr. J. B. Berkart (*British Medical Journal*, November 25, 1893) claims that, in the usual conditions in which defective drainage is supposed to exert its baneful influence, it is impossible that pathogenic micro-organisms which may exist in an untrapped pipe or in a cesspool can escape into the air. The force of evaporation is not enough to lift from a moist surface an organism, however small it may be; and even powerful ascending currents of air can not convey from a dry and porous soil, much less from a cesspool, any germs. Consequently, from untrapped pipes and cesspools nothing but irritant and toxic gases can escape.

He experimented through six or eight hours with currents of air at a velocity of from twenty-two to forty-five miles an hour. They did not lift into the atmosphere a micro-organism from a putrid solution of extract of meat of not more than a half per cent., or from putrid urine, and were unable to detach a micro-organism from any such putrid solution as may have been allowed to dry on the walls of a glass vessel or on wire gauze.

The question whether any, and which diseases, can be produced by the inhalation of sewer air has engaged the fears of a great many and the attention of a number of observers. A careful contribution to the literature of the subject is that of H. Hun (*Medical News*, August 20, 1887). He admits the absence of proof of a direct infection by sewer gas, but has quite an array of cases of ailments and diseases attributed to it. Anorexia, constipation, vomiting, diarrhœa, and coated tongue are frequent; prostration, drowsiness, headaches, small pulse, delirium, clonic and tonic spasms, fever, chill, and coma, intercostal neuralgia, Bright's disease (though in a person of sixty-five with arteriosclerosis, and another of sixty years); poliomyelitis in a patient of twenty-nine, who never recovered fully, and of forty-two who recovered after two years; also enlargement of the spleen, with albuminuria, are among the observations made in persons exposed to the exhalations of sewers or cesspools. It will be noted, however, that among all these cases there is not one which can be traced with the knowledge we now possess to a specific germ.

Mark Style (*Lancet*, October 19, 1889) attributes cases of acute pemphigus to the inhalation of sewer gas. Two children of five and of two years lost color and felt drowsy for a fortnight, then developed blebs on feet and shins; new attacks occurred on other parts of the

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body (no erythema with it). There were fever and anorexia. The sewers were found to be badly constructed and leaky; when they were mended the children improved.

Hæmoglobinuria in a child of eight years, "probably due to the inhalation of sewer air," was observed by Gordon Sharp and William Summerskill (London *Lancet*, Dec. 9, 1893). The girl lived in comfortable circumstances, was previously in good health, fell sick with dyspnœa, puffy appearance, and frequent micturition, which resulted, however, in six ounces daily only of a chocolate-brown urine the sediment of which rose to the top. Guaiacol and ozonic ether gave the characteristic blue color. There were no casts, only a trace of albumin, but few blood cells, much amorphous hæmoglobin. Convalescence began in three days, hæmoglobin disappeared after the fourth day, and anæmia remained behind. In the dwelling the water-closet pipes had been leaking, and the smell had been disagreeable. The pipes were being changed, and the smell was worse when the attack came.

It appears, in the opinion of the authors, that sewer air affects young children quite rapidly, and noxious vapors are known to produce hæmoglobinuria, but it is claimed that no previous case like the above is known.

In the experience of Dr. A. H. Smith, the president of the Climatological Society, in 1881, a large number of the attendants in St. Luke's Hospital, New York, were sick with tonsillitis. Examination showed that the brick sewer which ran beneath the building had fallen in in many places, and the sore throats ceased when iron pipes were substituted for the brick sewer.

The same gentleman communicates to me the following facts:

At Elberon, N. J., in the latter part of August, 1891, occurred a series of eleven cases of sore throat within a period of eight days in the summer residence of one of the most prominent of the cottagers at that place.

The first person attacked was the butler. He complained of great soreness of the throat and severe headache, but continued for two or three days to wait upon the table. When first seen there was intense congestion of the tonsils and fauces, but no membrane, and no exudation at the mouths of the tonsillar follicles. There was little or no swelling of the throat, and no enlargement of the lymphatic glands. The character of the throat lesion remained the same throughout the attack. The temperature never exceeded 103° F. There was extreme lassitude and severe aching of the bones. This, which was the severest case, lasted about ten days, the dysphagia being the most prominent symptom to the last.

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In rapid succession ten other inmates of the house, including several guests, exhibited similar symptoms in varying degrees of severity; in two of the cases the throat lesion was that of follicular tonsillitis of a mild type. In the other cases there was simply a dusky redness of the fauces, and some degree of pain in swallowing, lasting from three to six days.

It was discovered, as the result of a sanitary inspection of the dwelling, that a bath-tub on the third floor had been for some time disused, and that the trap had become dry, permitting direct communication with an old cesspool, the existence of which was not known. No other plumbing of the house discharged into this reservoir, and there was no offensive odor from it.

No communication of the butler with any source of infection could be traced, but the negative evidence on this point was not conclusive, as unconscious exposure could not be wholly excluded.

The bath-tub and its connections were removed, and the house has been occupied for two seasons since without the occurrence of further trouble.

Earlier in the same summer a group of four similar cases occurred in a house about half a mile from the one just mentioned. The first patient was a young lady, in whom the throat lesion was similar to that of the butler already referred to, but with the difference that the throat was extremely painful even when at rest, and the dysphagia was so great that the patient could scarcely be prevailed upon to take even the smallest amount of nourishment. The fever in this case was moderate, and there was no aching of the limbs.

Three other cases occurred in the house within a week. One of these showed well-marked follicular inflammation; the other two only engorgement of the mucous membrane, chiefly venous in character.

Examination showed that the main waste-pipe, which ran under the house for nearly the whole length of the latter, was of clay, and was broken in numerous places. The soil along the whole length of this pipe was saturated with sewage. An iron pipe was substituted, and the contaminated soil removed and replaced by dry sand. No sickness has occurred in the house during the two seasons that have succeeded.

Owing to special reasons, there was absolutely no inter-communication between the persons constituting these two groups.

One of the latest contributions to the same subject is a book on the combat with infectious diseases by Brix Pfuhl and Nocht (*Die Bekämpfung der Infectionskrankheiten*). After discussing the necessity of access of air to a sewer, to prevent it from getting putrid and giving rise to bad odors and dangers, they say on page 310:

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"The transmission of infectious diseases by sewer gas has not been proved by past researches and may be considered as out of the question. But bad sewer air can produce nausea, headache, and malaise (when its effect is persistent), and may become one of the causes of other morbid symptoms. To the workmen employed about sewers the preservation of pure air is of paramount importance. Thus successful aeration has a great hygienic importance.

"Investigations referring to the health of men employed in sewers had negative results. They do not suffer more than the average population from infectious diseases.

"Only those employed in the sewers of Wiesbaden suffered from rheumatic complaints more than other public employees. This was due to the fact that the Wiesbaden sewers, carrying off the water of the hot springs, have a constant temperature of 25° C. or more, thus exposing the workmen to frequent colds. Thus their rheumatism depended on circumstances not at all connected with sewers or sewer air."

To my mind the assumption that throat disease and sewer air must be connected with each other is probably due mostly to the irritability of the fauces. Pungent odors and tastes are not tolerated, chloroform can not be administered to a sleeping person because of that circumstance, and strong gases produce cough and discomfort. Hence irritation, hyperæmia, and catarrh may well be explained by the contact of malodorous and sharp gases with the vulnerable mucus membranes of the throat, particularly of children, but specific germs and toxines are, unfortunately, not malodorous, not pungent, and not irritant, locally. Indeed it is in this that lies their principal danger.

Compared with the frequent endemic occurrence of sore throats under the apparent influence of sewer exhalations, which is suggested by some of the reports, I am permitted to make use of a report made to H. M. Biggs, M.D., Chief Inspector of the New York Health Department, by A. Clinton, M.D., Inspector. The report is a very careful one, and the one thousand cases of throat affection detailed under the heading of pseudo-diphtheria, which occurred, or rather were reported, from August 1, 1893, to April 1, 1894, in the city of New York from the Battery to East and West One Hundred and Twenty-fifth Street have been accurately located on large city maps. The principal conclusion to be drawn from these two maps, kindly intrusted to me by the Health Department, for whose coöperation in the preparation of this paper I am thus greatly indebted, is this, that to the best knowledge and belief of the experts of the health department the occurrence of throat disease, particularly false diphtheria, is

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in no way connected with sewers, open sewers, leaky sewers, or outlets of sewers. The same conclusion must be drawn—I may say that just here—from two other maps placed at my disposal which prove that there is no connection in New York city between diphtheria and sewer air in any shape or form. In the latter instance there can be no doubt whatsoever, as the reports of diphtheria cases must be supposed to be correct.

There is, however, some evidence in the practice of every medical man and in public statistics that sickness in general, and fevers—mainly typhoid—coexist with the accumulation of excreta and other refuse material, though no infected water be drank. Definite amelioration has invariably followed their regular removal. The facts carefully collected by Dr. Buchanan in his *Ninth Report of the Medical Officer of the Privy Council* prove a considerable lowering of the death-rates by such amelioration, particularly in typhoid fever. The same result has been obtained from the same cause (*i. e.*, improved sewerage) in a number of cities—such as Salisbury, Bristol, Carlisle—where the sewers are ample and well ventilated.

He asks: "Why is it that some cities, like Chelmsford, Penzance, Worthing, and Morpeth, with ample sewers, have an increased mortality of typhoid? In one, 'the sewage is delivered into a tank by an outfall sewer which enters some six feet below ground,' with the result that when the engine is not at work or the liquid accumulates in the well, cellars get flooded by the sewage, and sewer gases get forced up into the houses (W. H. Corfield, page 252), The Treatment and Utilization of sewage, 3d ed., 1887. In another one there was no ventilation of the sewers, and sewer gas was forced back through the traps of sinks and water-closets. In another the pipe sewers are below the level of the river, so that in times of flood the sewage is backed up into the main sewer for four or five hundred yards. It is known that outbreaks of typhoid followed times of flood when the outfall sewer had been under water."

The general reduction of mortality can not be said to have extended to infants under a year to the same degree. Typhoid fever is not frequent at that early age, and when it occurs, it is mostly mild, and few deaths occur from that source.

The mortality of infants depends on different circumstances. Diarrhœal diseases do not appear to have been visibly benefited by im proved sewerage.

Scarlatina, measles, whooping-cough, croup, and diphtheria were not rendered milder or less fatal through improved sanitation in general, and sewers in particular. On the contrary, both scarlatina and

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diphtheria were greatly increased; on the other hand, "cholera epidemics appear to have been practically harmless in the towns examined" (page 47). Even pulmonary phthisis exhibits a great general reduction of its death-rate wherever, but only there where pipe-sewerage was accompanied with measures taken for the purpose of drying the subsoil generally, such as a special system of deep rain-water culverts.

Stevenson and Murphy, page 11 of their treatise on *Hygiene and Public Health*, refer to the enteric fever in Eastney Barracks, where sewer air was forced back by the tide into the drains, which had no traps but many leaks. When traps were put in and the leaks mended, the fever subsided. Edward Seaton (*British Medical Journal*, December 23, 1893) refers to his experience with dry earth closets. When they were largely introduced after the abolition of privy vaults, the mortality from typhoid fever was greatly reduced. The English generally believe firmly in the dependency of typhoid fever on cesspool and sewer exhalation. It is true that typhoid is apt to be more frequent where there are no sewers but cesspools, but the former are but cesspools rendered entirely or mostly innocuous by their structure and isolation. If the covers were removed from the sewers they would be open conduits, in fact cesspools, and worse than mere privies.

I believe I am correct when I say that the large majority of cases of typhoid fever we observe in New York city in September and October of every year are imported from the country. There a large concourse of people takes place, larger from year to year, in farm-houses, boarding houses, or large hotels. Many of these cases can be traced to the same place, not infrequently big hotels with a good reputation. The cases have become more numerous from year to year, and just at the time when the people thus stricken expected to be benefited by their summer outing.

Cesspool poisoning I know exists. The following case is an instance: A lady who annually took dozens of her boarding pupils to the country, hired for that purpose large farms or country places. During the whole summer not one of the children and young ladies fell sick. One day she happened to come near where workmen were emptying a large cesspool at a great distance from the dwelling and the drinking-water supply. She was exposed to the disgusting odors but a few minutes; but not being acclimated, within ten days she came down with a very severe typhoid fever, the only case in the whole community. Typhoid fever in dwellings in which the water-closets were in disorder, mainly those located in the interior of houses,

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I have met with in a number of instances. In the same way, and from the same cause, I have seen dysentery. In several instances I have seen tenement houses full of dysentery, where I could convince myself of the unusual filthiness and offensiveness of the common privy in the rear of the place. In ill-kept sick-rooms or hospital wards, where dysenteric stools are not disinfected and removed, dysentery will spread.

There are many authors who go far beyond this. Dr. George Cordwent (*British Medical Journal*, November 25, 1893) is more positive than most of those who like him believe in the direct production of infectious and contagious diseases by defective drainage. He is even anxious to substitute "privy odors" for sewer air, and takes it for granted that bad drainage "frequently evolves gases producing typhus, diarrhœa, etc., often *without* diphtheria; but of forty-three cases of diphtheria—all rural—all occurred in houses pervaded by a strong privy odor." He adds that this is a condition then quite usual in the laborer's cottage, but does not say why there were, in the course of three years, 1858–'60, not more than these forty-three cases, nor why there were none of them before those years. The real explanation of it all is that there *were* cases of diphtheria when the mucous membrane, affected by the influence of gases, was invaded by diphtheria germs, and there were none, in the same prevailing conditions, when there was no diphtheria about. At all events, however, it is a grave mistake to consider "privy odors" and a specifically infected atmosphere identical. They are far from so being. No specific germ has an odor.

R. W. Parker commits himself unmistakably in favor of sewer-air borne diphtheria. His reasoning is simple. For fifteen years he met with cases in which the infection appeared to come from drains. The special infective material has got into the drainage system. There will be more and more diphtheria, since the main drainage system is now largely ventilated directly into the open streets. The ventilators, while they protect the individual houses, poison the whole atmosphere and spread the disease broadcast. It is this cause to which the increase of diphtheria in London is said to be largely due. So in his opinion the ventilation of the sewers is a great danger. It would be so if the ventilation of a sewer be defective; if, for instance, there were but a single outlet to the subterraneous cesspool, the odors and exhalations would be disagreeable, and possibly harmful. Diarrhœa, nausea, vomiting might be and are produced in those exposed to the odors—children with vulnerable mucous membranes and respiratory organs only two feet above ground would be principally endangered—

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but unless there were a specific germ, or rather a number of germs, in the exhaled air there would be no specific disease. If there were any admixture of specific germs, they would be, the worse the odors of putrefaction, the sooner destroyed and rendered innocuous.

In the *Lancet* of January 13, 1894, H. Grant Sutton, M.D., relates the following incident: On March 25, 1892, diphtheria broke out, and afterward spread, one hundred and fifty yards from a quay on which immense masses of decomposing animal and vegetable matter, rags, woolen materials, old chair seats, feather beds, and all kinds of rubbish and filth were burned and lay smoldering for weeks. "Though the gases are volatilized their poisonous properties are not destroyed, and so these noxious fumes are carried in the direction favored by the wind, one of which would probably be over the village where diphtheria did actually occur." Now, the report is not positively clear as to whether the refuse of the city is actually and completely destroyed by burning or not. Even if it be so, depositing and the burning are not simultaneous, and it appears very much more probable that the wind carried diphtheria germs directly from the huge masses piled up. Is that possible or probable? I believe it is. In spite of the great diffusibility of gases, they are liable to remain in a solid mass. Our tornado experience proves that moved air passes through many miles with sharply defined boundaries; and standing on board a steamer we see the masses of smoke and steam cling together, even for miles, in an unbroken column. So it is quite possible for the wind, if it be strong enough, to carry disease germs to a distance. But the coöperating requirements of such a result must be the ample presence of pathogenic germs, the absence of disinfectant sunshine, and wind blowing sufficiently strong in one direction.

Begging the question and absence of logic are great aids in making mistakes and perpetuating popular prejudices. In the coolest manner possible Nicolaus Gerzetic (*On Parasitism and Disease Producers*, 1893, p. 96) delivers himself as follows: "It is well known that sewer air carries pathogenous germs, like those of diphtheria and gastro-enteritis, even typhus, according to Buchanan and others."

George Carpenter, M.D., of London (*Arch. of Pæd.*, May, 1894), goes further than his predecessors. He attributes a case of diphtheria occurring in an airy house located in a healthy district to the inhalation of the foul air emanating from a putrescent placenta which had been left in a night commode at the head of the bed. He means to prove that decomposing animal and vegetable matter can be readily and very reasonably accused of inducing the disease, and besides declares it probable that the diphtheritic virus has found a suitable

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nidus in these decomposing materials for its growth and propagation, and the engrafted virus is the real cause, though not the apparent one. "In the same way sewer air acts as the vehicle and not the cause." "By a rational process of thought we feel that this must be so, but sometimes a case crops up to which our reasoning does not apply, and we are left wondering as to how a decomposing mass of matter could become infected."

I should say it could not; for if there be a power that destroys pathogenous germs it is putrefaction.

In many instances the reasoning in this matter is simply emotional. Thus, according to the *Massachusetts Association of Boards of Health, Official Journal*, September, 1893, page 23, mention is made in the discussion on a recent law passed by the State Legislature of Massachusetts on the licensing of plumbers of Mr. Roe, of Worcester, who introduced the bill and worked for it. He is mentioned as "a man who had difficulties to encounter in his own house regarding unsanitary conditions. His child died of diphtheria, and the house was examined by an inspector of our board and found to be in an unsanitary condition. At the time he was loath to accept it as the cause of the death of his child, but eventually he felt convinced. He is an ex-principal of our high school here and a broad-minded man, and is thoroughly convinced of the fact that the child must have died from the effects of the unsanitary conditions." This is the kind of report that will influence public opinion. Emotional reasons in place of scientific reasoning will always carry the day. *Credo quia absurdum est.*

Some of the medical reasoning is not much better. For instance, Barnes (*Brit. Med. Jour.*, July 28, 1888) studied fifty separate outbreaks of diphtheria for the purpose of discovering if diphtheria may not arise from certain combinations of filth and unsanitary conditions independently of a pre-existing case of the disease. According to him the prevalence of diphtheria in rural districts is explained by the want of suitable systems of drainage, combined with filth in the form of decomposing animal matter. In the majority of instances he found no previous case as the starting point of each outbreak. Besides, the author claims that the winter months, when most diphtheria is met with, are unfavorable to the development of low forms of animal or vegetable life—he forgets that the throats and the houses are warm—and believes that because sore throats existed previous to and simultaneously with the outbreaks of diphtheria, it follows that we have to deal not with a specific germ but with a poison gradually developed.

In the *Medical Record* of January 28, 1893, Dr. Louis Fischer approached the subject a little more seriously. He published a very

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interesting article on The Result of Examinations of Sewer Gas which Escaped in Tenement and Private Houses wherein Cases of Diphtheria Occurred. It is to prove that escaped sewer gas will easily enter houses and poison their inmates. As a single positive proof would prove more than a thousand negative observations would disprove, I have carefully read and reread the paper. I can not learn, however, that his conclusions are unimpeachable. A child had diphtheria; the house was a tenement, four families on a floor; in the previous winter there had been diphtheria on the floor above and the floor below on the same side of the house. The doctor says "the disease broke out successively on different floors and was seemingly spread by way of the sewer pipe leading up through the house. On the other side of the hall diphtheria did not break out."

He then describes the deterioration of the air in the room, due to the closing of windows in the narrow quarters and to the proximity of the sink into which sewage and refuse was emptied, and adds that underneath this sink there was a trap which served as a reservoir and to exclude noxious gas coming from the sewer.

He does not say that the trap was defective, nor that there was any reason why the disconnection of room and main sewer should have ceased. He simply assumes that because there was diphtheria above and below in the previous winter, there must be a communication of the germs through the pipes into the intermediate floor, which, however, was protected by a trap a year afterward.

Moreover, he alludes, himself, to the fact that what is thrown down into the sink and the reservoir underneath may be deposited there and, under favorable circumstances, may there find a convenient culture medium. Still more, he reports that he found bacilli in the air of a room where diphtheria existed, and in an adjoining room (not connected by a waste pipe at all); also that he made a series of examinations of air in a number of other houses with the following results: Of eighty-five experiments performed, forty-five must be excluded owing to carelessness and breakage of plates. Of the remaining forty, twelve yielded pathogenic bacteria; eight, different micro-organisms (non-pathogenic); and twenty were negative. Thus, in thirty per cent. pathogenic bacteria were found in miscellaneous houses, which merely proves that pathogenic germs are ubiquitous and waiting for chances. These chances are *reduced general health, diminished power of cell resistance, and denuded surfaces.*

After Dr. Louis Fischer had looked for dangers "in the gases arising from sinks," and emphasized that "sewer traps themselves may become, if not properly flushed, breeding grounds for bacteria," Dr.

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F. W. Koehler claimed at once that "defective sewer pipes, dampness, and lack of cleanliness account for most cases of diphtheria occurring in private practice" (The Prevention of True and False Diphtheria, *Medical Record*, September 30, 1893). The better part of his paper *may be repeated here*, not because it is new, but because it is true, "The waste pipe of a washstand or sink may be a source of infection. I refer to that part of the pipe between the external part of the external opening and the nearest trap. This section of the sewerage gets the warmth of the house, has quantities of organic matter poured into it, has water more or less constantly running through it, always contains air, and is consequently a most perfect culture ground for many kinds of bacteria. The traps may therefore be in perfect order, and yet we may not be safe. Some device to shut off also this part of the sewerage system from all communication with the atmosphere of the house seems to me badly needed." In reply, I should say: disinfect sink and trap once a day.

In all this question of cesspool and privy exhalation, what we know tolerably well is this, that it may result, as it frequently does, in a pharyngitis—the pharyngeal and naso-pharyngeal mucous membrane being exceedingly irritable—and that bacilli, being ubiquitous during the reign of an epidemic, may find a nest in the sore surface.* The *Bacillus diphtheriæ*, in particular, is not in the cesspit and has not been found there, for putrefaction deals as inclemently with bacilli as with other organic matter.

There is, however, no doubt as to the difficulty encountered when the origin of an individual case of diphtheria is to be explained. The following case will give an instance of the various ways in which diphtheria may arise without sewer gas or spontaneous generation.

Dr. W. W. Ralston, of Horton, Kan., communicated to me the case of a boy who died in a neighborhood where no case of diphtheria had ever been known to exist, who lived under the best possible circumstances—good drainage, good air, no contagion, good health. Two years previously a child died of diphtheria in Chicago, where the family then lived. Afterward they lived in Detroit, later they moved to Kansas, and with them traveled a swab which had been used on the diphtheritic child in Chicago. When the little boy was (in Kansas) taken with a suppurating amygdalitis, the mother bethought herself of her swab, and used it to apply an alum solution. Thus it was that her boy died (*Arch. Ped.*, 1889, p. 131).

* It is a common experience that in a family of children such as are suffering from a catarrh will be taken with diphtheria if any.

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In the face of such a fact the weight of such cases as will now be quoted is not of much account.

Dr. Philip Francis Harvey (Fort Keogh, Montana) publishes two cases of diphtheria from "faulty drainage" (*Lancet*, 1892, i, p. 1184.)

The first case was that of an officer of the United States army who had not been exposed to any known source of contagion. The case proved to be very malignant, and terminated fatally by heart paralysis after about eight days progress. One week later the patient's wife developed a fatal attack. The disease originated *de novo* at an isolated military post in a new brick building. The location of the post was high and dry on a plateau between the confluence of the Mississippi and Minnesota Rivers.

A thorough sanitary inspection of the house was made, and a faulty drain was found in the basement with some leakage of sewage into the earth at that point. Here, then, was the explanation of the origin of the disease, and a demonstration how rapidly the diphtheritic poison may *be formed* under circumstances *favoring its evolution*, as the cases occurred in January and the premises were first occupied the preceding November.

Another case was reported, under the same heading, by Dr. H. Mallins (*Lancet*, 1892, i, 579). It was that of a boy ten years of age, who was attacked with membranous sore throat, followed in a few weeks by almost universal paralysis, a circumstance that proved its true diphtheritic nature. The boy slept in a small room directly over the scullery, and for weeks past a most unpleasant smell had pervaded this room. The scullery sink was found to communicate by an untrapped—in places broken—pipe with a cesspit situated about twenty-five yards from the house. There was, therefore, every opportunity for the foul air from the cesspit to be siphoned into the house. The patient was carefully isolated, and, though he was one of a very large family of children, no one else caught the infection. As there was not a single case of the disease in the neighborhood, as the boy did not mix with any children except his own brothers and sisters, and lastly, as he was the only one in the family sleeping in the line of escape of foul air, the conclusion seems irresistible that in this case, at all events, the diphtheritic poison was conveyed in the emanations from a foul drain, such emanations polluting the air that was nightly breathed. How the specific bacillus got into the cesspit is, of course, a very difficult matter to explain.

The possible causes of an invasion of diphtheria are so many that a resort to an autochthonous origin ought to appear superfluous. Perhaps, however, it is the very multiplicity of possibilities which acts

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confusing and bewildering: the vulnerability of the young mucous membrane, the frequency of nasal and pharyngeal catarrh, the narrowness of the nose, the large size and softness of the tonsils, the frequent fermentation of food in the mouth, the sucking of the soiled little fingers, together with the influence of family disposition, which is more powerful in the young, their constant intercourse with each other in large families and in densely populated houses and districts, in schools and on playgrounds, the possibly long period of incubation during which the disease is contagious though giving rise to no symptoms, act as just so many predisposing causes of contagion; and the large number and size of the lymphatics renders every attack so much the more dangerous.

The very fact that diphtheria need not always be of the same type; that many cases of lacunar or follicular amygdalitis ("tonsillitis") are diphtheritic—a fact proclaimed by me dozens of years ago, which I have the satisfaction of seeing more and more, though with great hesitation at first, established even by bacteriologists—that there are as many cases out of bed and out of doors as in bed and indoors; that, particularly in adults, diphtheria may last long and give rise to but few embarrassing symptoms, and that a mild case of diphtheria may produce very serious ones by contagion, renders contagion by nursery maids and other domestics—by teachers, seamstresses, sick-nurses, workmen, factory girls, shopkeepers, barbers, and all other persons mingling with the many extremely easy. The persistent vitality of the diphtheria germs, as is well known, may extend over years. They cling to solid and semi-solid bodies, are imported in milk, cling to walls and floors, to toys, to curtains, towels, clothing, and bedding which is so often kindly donated to the poor by the benevolent well-to-do when they wish to get rid of their dangers. They stick to omnibus and carriage cushions, to rail-car seats, to the either ready- or custom-made coat on your shoulders near which your baby will nestle—the very coat that is sold in Broadway palaces after it has been made in the pest-stricken tenement sweating shop. The very restlessness of our people, the frequency of moving, is another cause of doubling the number of cases. There can be no doubt, besides, that many animals —horses, chickens, cows—have and spread diphtheria. Thus it appears that we ought to think twice, and indeed many times, before admitting among the causes of diphtheria new factors which can not be proved.

"No contagion could be traced." That is the introduction to every wild and unproven theory of indigenous spontaneous generation. When a case of cholera breaks out in a village a thousand miles away

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from the coast, is there anybody in our time who looks after chemical poison in a well or for filth on the roofs? You look for direct or indirect contagion from a tangible source. Why not so in diphtheria? In the *New York Medical Journal* of September 27, 1886, I have quoted from Isambert the case of a medical assistant who had nasal diphtheria many months, and then traveled half a year to get rid of the last remnants. He fully recovered; but how many deaths did he spread—from railroad car to railroad car, from stagecoach to stagecoach, from hotel to hotel? How many may have been the physicians who searched in vain for the causes of the sporadic cases suddenly springing up in their towns, and the epidemics generated by them along the roads on which the luckless French wanderer after his own health strew out his curses? Nobody suspected the traveler who left days ago, just as nobody may be able to trace every outbreak of cholera to the unknown person who carried it upon his person or in his bowels. Nor is this an isolated case of a long duration of diphtheria. Cadet de Gassicourt operated for laryngeal diphtheria after eighteen, twenty-three, and forty-three days. Sanné had croup patients who recovered after twenty-seven, thirty-two, and sixty days. I know of many cases of diphtheria protracted into the second or even the third month.

Such facts, pointing as they do to the ready communicability of diphtheria, have influenced my opinion from early times. I can not see anything miraculous in the sudden appearance of a *Bacillus* or a *Streptococcus diphtheriæ* in a person *apparently* not exposed to it. During an epidemic there is *nobody not exposed* to it, and everybody is subject to it under favorable circumstances. The latter mean a fit condition of the human integument, either cutis or mucous membrane, which makes them liable to become a resting place for the germ. That fit condition is a slight or severe wound, abrasion, denudation of the surface. As no healthy surface becomes erysipelatous in spite of erysipelas being epidemic, as Fehleisen's bacillus requires a sore, so diphtheria, being ubiquitous and waiting for a chance, will stick to a cutaneous wound, a stomatitis, a pharyngeal or nasal catarrh, and will rapidly multiply. A resected tonsil will thus be covered with a pseudo-membrane within a day. Only yesterday Dr. Caillé reported in the meeting of the American Pædiatric Society such a case. It terminated in generalized (including laryngeal) diphtheria. Without the operation, that tonsil might not have been affected at all.

In my paper on diphtheria and diphtheritic affections (*Am. Med. Times*, August 11–18, 1860) I acknowledged contagion only as the cause of the origin of an individual case. In 1861,* Jenner, after an

* *Diphtheria; its Symptoms and Treatment.*

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experience of five years among the poor and the rich, maintained the independence of diphtheria from bad hygienic conditions, and stated even a larger mortality among the rich. In a lecture published in *Guy's Hospital Gazette*, 1873, Samuel Wilks reports that it spread from the focus in Folkestone along the eastern counties of England, apparently quite irrespective of soil, impure atmosphere or drainage. As regards London, it was more frequently met with in the better class of houses in the suburbs than among the lower and dirtier habitations of the poor. These views were not exactly refuted by good observations, but did not strike the fancy of the medical public. Jenner's view was strongly condemned in an otherwise favorable review contained in the *Dublin Quarterly Review*, August, 1861; nor was the conviction of the profession in this respect much changed before the appearance last year of Thorne Thorne's book, which again proves, as I tried to do thirty-three years previously, contagion as the ætiological influence, without relation to bad sanitary conditions as regards water supply, sewerage, and drainage.

In the discussion on the same question contained in the *British Medical Journal* of the two last months of 1893 and the beginning of 1894, Dr. Wilks again takes a leading part. His opinions on the same question have not changed. Davis, Priestley, C. M. Jessop, J. Bunting express themselves in the same way. George Johnson, Parker, C. N. Allfrey, H. G. Warrey (who assumes that every "membrane in sore throat is always diphtheritic"), and P. G. Marriott favor the sewer-borne origin of the disease; and George Johnson associates with diphtheria, in this respect, typhoid fever, pneumonia, puerperal fever, and albuminuria, which "may have such an origin," and charges that "those who believe in contagiousness only will not look for bad drainage." A few of his cases, which are to prove his point of view to be correct, are the following: In a wealthy house an infant was circumcised for phimosis; the wound became diphtheritic, and did not heal until the patient was removed to another house. Two servants were also affected with diphtheria; after defects in the sewer pipes were found and corrected no other case occurred. Another case is that of a butler in a wealthy and a healthy house who contracted diphtheria. An untrapped sink-pipe was discovered near his sleeping-room. He recovered, and there was no other case after the defect was mended. More, however, we do not learn, particularly nothing to exclude the hundred possibilities of contracting the disease.

If you will permit a personal remark I should here say that in spite of my positive statements, repeated a dozen of times in writing,* and

* With the exception of a careless expression on page 50 of my *Treatise on Diphtheria*, which is quite in contrast to my previous statements in the same chapter.

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a hundred times in lectures and discussions, I have personally been claimed as favoring, in the case of diphtheria, the sewer-air theory. As late as this year, Emil Feer (*Aetiologische und klinische Beiträge der Diphtherie*, 1894) says (page 67) that "both in England and America there is a prevailing opinion, both among the medical men and the public, that uncleanliness of dwelling and people is a main cause of disease; in these countries diphtheria is often called a filth disease. According to Jacobi the connection between diphtheria and filth has, it is true, not been proved, but the author states as the result of his conclusions that it exists; for there are many reports which exhibit the co-existence of diphtheria and filth." A few moments later he gives me credit for a teaching which is also opposed to life-long convictions, for he adds: "The injurious influence of sewer gas and cesspools is emphasized by Baginsky, Monti, and English and American authors (Jacobi)." Of my writings, he quotes only my article on Diphtheria in Gerhardt's manual, 2d volume, of 1877. The only allusion to sewer gas contained therein is found on page 703, where I say that the influence of the seasons on the origin and the course of diphtheria is but conditional and indirect, in a similar way as that of "filth," or sewer exhalation.

In the November number, 1888, of the *Archives of Pædiatrics* the late Dr. Charles Warrington Earle published a brief article on "the influence of sewerage and water pollution on the prevalence and severity of diphtheria." He begins by saying that "it has been claimed by many that imperfect sewerage has been the cause of diphtheria, and the people, urged on by the opinion of the doctors, frequently blame a sewer for poisoning a family and producing diphtheria, when the cause should be placed elsewhere. It is much better for us to recognize the true cause, if it is possible to find it, rather than to attack an imaginary one, for it is possible that while we are fighting the supposed gas as the cause, we are losing sight of the real enemy which should engage our attention."

"Jacobi says that cases of diphtheria which are traced to exhalations from sewers, or even to filthy habits of life, are very frequent. This opinion, especially in regard to sewerage, has been reiterated by scores and hundreds of physicians. It represents the prevailing idea of American physicians." Then Dr. Earle prints the opinions of a number of correspondents who deny the origin of diphtheria from sewerage, and shows me the errors of what he believes to be my ways.

Now, as it is both unhistorical and unpleasant to be charged, after a life in part spent on the study of diphtheria, with the very erroneous views always combated by me, I tried to refute Dr. Earle's allegations

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in the November number, 1888, of the *Archives of Pædiatrics* (page 744). As my letter to the editor contains in the briefest possible way a statement of what I believe to be the facts, I beg to here repeat them.

"In my very first paper published on the subject (Diphtheria and Diphtheritic Affections, *Amer. Med. Times*, August 11, 1860, p. 96), I looked for the source and epidemic occurrence of diphtheria in contagion to the exclusion of any and all other alleged causes.

"On page 34 *et seq.* of my *Treatise on Diphtheria* (1880) you may read these remarks: 'Cases of diphtheria which are traced' (I might have said attributed) 'to exhalations from sewers (or even to filthy habits of life) are very frequent. Yet typhoid is attributed to the same causes. So is dysentery. Can these foul exhalations produce alike diphtheria, typhoid, and dysentery? Do these diseases arise from a common poison? Or is the poison of a treble character, so that a part may give origin to diphtheria, the other part to typhoid, the third to dysentery? In a house in West Twenty-second Street, between Eighth and Ninth Avenues, in New York, three children and a domestic were taken sick, two with dysentery and two with typhoid, in the course of a month. In the same house, in two of the children diphtheritic sore throats were of frequent occurrence.

"'Have we to deal in such occurrences with special influences, or only with a lowering of the standard of health, thereby affording other morbid influences an opportunity to exercise their power?'

"I then quote (page 35) the results of the researches of the Board of Health of the State of Massachusetts, the third of which reads as follows: 'A positive connection between diphtheria and filth can not be verified, although the latter adds to the evil influence of moisture.' This statement I call (page 36) 'modest and sensible,' and one 'for which we have to be thankful.'

"In Diphtheria Spread by Adults (*New York Medical Journal*, September 24, 1884) I say: 'No permanent spontaneous generation is claimed or has been proved for cholera, scarlatina, or variola. Nobody looks for their primary cause in moist walls of houses, dry dust of streets, in the prevalence of previous house endemics of typhoid fever, measles, or other eruptive diseases, in bad ventilation, in the odors of hospital wards, in putrefying kitchen refuse, or in the exhalation of sewers. But both medical men and laymen are found to be inexhaustible in accusing and condemning all those detrimental influences, not as being predisposing elements, not because of their injurious influence on health in general and on the condition of the mucous membranes in particular, but as the main and frequently sole

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causes of diphtheria. In the minds of many physicians, diphtheria is intimately linked with sewage; with them the trap of the water-closet and the plumbing of the cellar are the first objects of attention, the patients and their families, fauces and nares coming in for a relatively smaller part of their care. If they would pay more attention to the direct sources of contagion, which is something understood and definite, than to the indefinite and unproved presumption of specific poisons in the outlets of the house or the inlets from the sewers, their ætiology would be something more positive in a great many cases.

"'I do not mean to say that the house hygiene ought not to be looked after by the physician in every case of sickness, but the more I have seen the more it has occurred to me that we may live to reach the conviction that there is but one predisposing element, viz: a sore mucous membrane, and but one cause of an individual attack of diphtheria, viz: direct contagion.'

"In a paper on The Therapeutics of Diphtheria, read before the Medical Society of Philadelphia, May 23, 1888, which has been copied by half a dozen medical journals, I made the following remark (reprint, page 1): 'Diphtheria is a contagious disease. There is probably no spontaneous origin of diphtheria, any more than there is a spontaneous origin of cholera or scarlatina.' And again (page 3): 'When an attack of diphtheria has made its appearance, it is well enough to examine the hygienic condition of the house, with its deteriorating influences on the general health of the inmates, but look after the source of the case in the persons of friends, attendants, and help.'

"In my Remarks on the Nature and Treatment of Diphtheria, made by invitation before the Section of Diseases of Children of the British Medical Association, August, 1888 (*British Medical Journal*, September 22, 1888), there are found the following sentences: 'Foul air and sewer gas do not create diphtheria; they do create dysentery and typhoid, or such a condition of general ill health and malaise as to afford the diphtheritic virus a ready resting place. There were plenty of malodorous privies and foul smells fifty years ago, but no epidemic of diphtheria. Besides, and mainly through the careful observations of English physicians, such as are contained in Dr. George Turner's report on diphtheria in the lower animals and many others, the sources from which diphtheria may come are very many. Pigeons, fowls, turkeys, chickens, pheasants, cats, horses, sheep, cows are just as many sources of diphtheria for man. Food of all kinds, vegetables and milk, will transmit it. It sticks to furniture, floors, and wall paper, railroad cushions and school desks. No spontaneous generation is required to explain its ravages.'

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"These extracts, Mr. Editor, ought to prove that Dr. Earle does not stand alone with his views so ably discussed in your journal. Like him, I have always lamented the disposition of so many of us to look for the cause of an individual case of diphtheria in the wrong quarter. Like him, I have often found a professional brother inspecting traps and cellar floors, while the rest of the children of the family were permitted to play in the rooms and about the beds of those affected with the malady.

"I have never believed, nor do I believe now, that sewer gas *per se* is a cause or the cause of diphtheria. A sewer or a trap can convey diphtheria only when that particular sewer or trap has been infected with diphtheritic poison."

After these quotations and to-day's remarks, I hope I shall be counted among those who will rather look for the cause of diphtheria in a germ communicated, directly or indirectly, from a patient or his belongings than from sewer air. The latter can not be made responsible enough in an occasional case where the introduction of germs into the sewer or into the sewer air, and their presence in the pipes, and their introduction into a house and their presence there are proved facts. My convictions tally with the experience of those who have seen much.

In a conversation with Dr. J. D. Bryant, for many years health commissioner of the city of New York, I learned a number of interesting facts. Since 1873 there has been an incessant war against defective plumbing; traps were enforced, and since 1878 roof waste pipes. Since Dr. Bryant entered upon his office the earthen or brick connections of house drains with the street sewers were replaced by iron. In reference to every case of infectious or contagious disease a strict inquiry was required, on the part of the medical inspectors, concerning ventilation and sewers, even of the adjoining premises. Many inspectors who did not find or report actual defects were discharged. The general result of the investigation concerning the coexistence of an infectious disease and defective sewer was, however, negative; the number of cases where defects in joints and waste pipes were met with in such cases was limited. The large majority of diphtheria cases were found in tenement houses. Still, sewer and drain defects were more frequent in private houses containing two or three families than in tenement houses. No special class of people were mainly affected. Diphtheria was alarming in the city of New York before and after the Health Department was established. In answer to my direct question I was told that both diphtheria and typhoid occurred where plumbing was perfect, and were often not found where it was

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defective. Many typhoid cases were imported from the country in September and October. Dr. Doty related the case, and referred to many similar ones, of a baker who lived in Tenth Avenue with his family over an open earthen and brick sewer into which a privy found its outlet. There was no case of sickness. Physicians always looked for and searched for sewer gas or sewer infection; but the comparative statements of a number of inspectors were negative. Nor could it be said that there were more cases of diphtheria or other infectious fevers near the outlets of sewers or in the immediate proximity of stables.

Dr. William H. Park sends me the following abstract of a paper of his recently read:

"From the reports from a large number of cases of diphtheria it has been shown that bad drainage and bad plumbing have but little noticeable effect upon the spread of diphtheria in New York city. Though we may readily believe that the escape of sewer gas into a room might easily predispose to sore throats, and thus perhaps render persons more susceptible to diphtheria, the Health Department inspectors have not been able to find any cases where the infection with diphtheria came through bad sewer connections."

"The maps which we have in our possession show well how all the tenement districts of New York are infected, and not certain ones over old sewers or streams; also diphtheria is found about as frequently in the garret as in the basement."

Mr. Archibald Montgomery, a very intelligent master plumber of more than twenty years' experience, gives the following as the result of his observation:*

"Plumbers do not lose more time from work on account of sickness than do mechanics in general.

"Emanations from decomposing night soil, etc., may cause vomiting, but not diarrhœa. The effect is only transient.

"Typhoid or other fevers are not more common among plumbers than among other workmen.

"The accidents arising from entering sewers are the result of the presence of either illuminating gas which has leaked into the sewer from the gas mains, or of carbon dioxide formed from the decomposition of sewage.

"In places where the level of the sewer is below the general line, carbon dioxide is liable to form in large quantity.

"The inoculation of an abraded surface with sewage often leads to great local swelling, with implication of lymphatic glands and general symptoms of 'blood poisoning.'

* Communicated to me by Dr. A. H. Smith, the president of the Climatological Society.

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"There is no discrimination against plumbers by life insurance companies."

I may be finally permitted to add the oral testimony of more than a dozen European medical men, and dozens of Americans. Every one was asked by me: What do you know of the production of a specific germ disease out of, or through, sewer air? The uniform answer was: There is a general vague impression among the public, but I never saw a case, or could prove one.

Some of the conclusions to be drawn from this paper would be as follows:

The atmosphere contains some specific disease germs, both living and dead.

They are frequently found in places which were infected with specific disease.

In sewer air fewer such germs have been found than in the air of houses and schoolrooms.

Moist surfaces—that is, the contents of cesspools and sewers and the walls of sewers—while emitting odors do not give off specific germs, even in a moderate current of wind.

Splashing of the sewer contents may separate some germs and then the air of the sewer may become temporarily infected, but the germ will sink to the ground again.

Choking of the sewer, introduction of hot factory refuse, leaky house drains and absence of traps may be the causes of sewer air ascending or forced back into the houses. But the occurrence of this complication of circumstances is certain to be rare.

Whatever rises from the sewer under these circumstances is offensive and irritating. A number of ailments, inclusive, perhaps, of sore throats, may originate from these causes. But no specific diseases will be generated by them except in the rarest of conditions. For specific germs are destroyed by the process of putrefaction in the sewers, and the worse the odor the less is the danger, particularly from diphtheria.

The causes of the latter disease are very numerous, and the search for the origin of an individual case is often unsuccessful.

Irritation of the throat and naso-pharynx is a frequent source of local catarrh; this creates a resting place for diphtheria germs, which are ubiquitous during an epidemic, and thus an opportunity for diphtheria is furnished.

Of the specific germs, those of typhoid and dysentery appear to be the least subject to destruction by cesspools and sewers. These diseases appear to be sometimes referable to direct exhalation from privies and cesspools. Very few cases, if any, are attributable to sewer air.

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A single outlet from a sewer would be dangerous to general health because of the density of odors (not germs) arising therefrom. Therefore a very thorough and multiple ventilation is required.*

The impossibility or great improbability of specific diseases rising from sewers into our houses, protected as they are, or ought to be, by good drains and efficient traps, must, however, not lull our citizens and authorities into indolence and carelessness. For the general health is suffering from chemical exhalations, and the vitality of cell life and the power of resistance are undermined by them.

* The sidewalk ventilators in New York city are almost all obstructed.

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SEWER GAS A CAUSE OF THROAT DISEASE (OR THE EFFECT OF BAD DRAINAGE ON THE THROAT).

BY BEVERLY ROBINSON, M.D.,

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"A great deal of vague assertion has been made with regard to the effects of sewer gas, or as it is better called, sewer-air, upon the human system. While some look upon it as always poisonous, and often virulent, others consider it usually harmless, though unpleasant. The good health of workmen engaged constantly upon sewers is cited in favor of this latter view."*

The opinion just quoted manifestly shows that there is much of expressed opinion on this subject found in medical literature, which is based upon facts capable of widely different interpretation. In a tenement house, let one or more cases of throat disease be suddenly present, and the attending physician immediately suspects the condition of the plumbing. A sanitary inspector from the Health Board is called. Untrapped, filthy sinks, malodorous water-closets, a damp cellar containing accumulations of organic substances, animal or vegetable, in a state of decomposition and giving off effluvia, will usually be discovered. Not infrequently, in addition to these sources of disease, the peppermint or smoke test properly applied, will make known a leaky joint, or a perforation of the soil pipe, which allows sewer-air to permeate the entire building. Which of these conditions—or do all combined—occasion the throat affections which have shown a sudden outbreak, or been present more or less continuously? How difficult to decide in a satisfactory manner!

Whenever, besides the preceding conditions, we find over-crowding, poor ventilation, bad food, lack of personal cleanliness, over-fatigue, exposure, anxieties, poverty, excesses,—how can we properly estimate the rôle of each determining or causative factor of the throat disease in its exact proportion? Evidently in many instances the task is very difficult, not to say insuperable. And yet there are unquestionably numerous examples on record in which the impure air from sewers and cesspools has seemingly been the means of propagating diphtheria and other throat diseases. Instances, indeed, are not wanting which go to show that the poisonous decomposition of organic matter in sewers is in some way connected with the dissemination of these diseases.

* The Medical Record, August, 1888, p. 186.

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Dr. Chaumont publishes two cases in support of this opinion: One is that in which a number of children were attacked with diphtheria, apparently caused by a leakage of sewage in a part of the house immediately under the nurseries of the children. The other case was that in which by a defect in the sewer-ventilating pipe passing up close to the cupboard in a nursery, there was an escape of sewer-air into the room occupied by a number of children, with the effect of sickening the whole of them.

Dr. Wilson (Handbook of Hygiene, 1877, p. 71), remarks that "in country districts isolated outbreaks of diphtheria traceable to cesspool effluvia, are not at all uncommon. In these cases, it is generally found that there is a water-closet in the house which itself is badly ventilated, and that the closet drainage discharges into a cesspool which is completely covered up and only cleaned out at rare intervals. The consequence is that any gases generated in the cesspool have no outlet except through the water-closet and into the house, and hence result attacks of diphtheria, ulcerated sore throat and other badly defined ailments."

"From an inquiry instituted by the State Board of Health of Massachusetts in 1875, respecting the predisposing causes of diphtheria, its prevalence, etc., it would appear that this disease has been most severe in the rural districts where there are no sewers and where the drainage is generally very bad. In some of these places, the level of the ground water is high, and on this account when there is high water many cellars are damp or contain standing water. This is probably one of the worst sanitary conditions in connection with diphtheria. It helps perpetuate it by furnishing a breeding place for its disease germs, if they become implanted, and it also favors catarrhs and sore throats, which renders persons having them more susceptible to that disease. Besides the conditions referred to, the soil is often polluted from house slops and excavated privy pits." (Report of State Board of Health of New York, 1893, pp. 294–95.

The belief that the propagation of diphtheria is greatly influenced by contaminated air from sewers, privies, soils, etc., or in other words, by filth infection, seems to be gradually gaining ground; but at present evidence is not sufficient to justify a decided opinion." (Hygiene and Public Health, Buck, Vol. I, p. 589).

Perhaps these faulty conditions produce a sore throat of a benign character which affords a soil suitable for inoculation by the diphtheria germ when present in the air. (Osler, Practice of Medicine, p. 99.)

According to other writers, defective sewerage, dampness and lack of cleanliness account undoubtedly for most cases of diphtheria devel-

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oped in private houses.* Indeed, in some of these instances, the presence of the Loeffler bacillus, the true cause of diphtheria, has been shown in the gases arising from sinks.† We owe this demonstration to the researches of Dr. Fischer, who has shown also that these sinks are not properly flushed and that for this reason they often become the breeding place for these and other bacteria. In commenting upon Dr. Fischer's researches, Koehler states that the portion of the waste pipe of wash stands and sinks between the outer opening and the first trap is particularly objectionable for this reason. This portion of the pipe receives varying amounts of organic matter and water, which is exposed to and warmed by the air of the adjoining room. These are, of course, suitable conditions to produce an excellent culture ground for many kinds of micro-organisms. The traps of a house may therefore be in perfect order and yet we may not be safe from infectious sore throats. The condition just referred to is usual, and yet frequently cooks and others escape sore throats, at least for a time. When they are otherwise well, the chances are that they will escape; when they are ailing for one reason or another, they are more likely to contract a sore throat from habitual contact with the source of contagion mentioned.

Many people, both adults and children, before contracting diphtheria or other affections of the throat, have been weak or sickly. The vitality of their body cells is already lowered and they are thus rendered more susceptible to the action of virulent germs. Many agencies conspire to bring about this condition; such are notably, wet and cold, hunger, physical or mental depression, worry and severe shock. In many instances, diphtheria, follicular tonsillitis, and other forms of sore throat have been known to follow exposure to some of these causes,—and independently, as far as can be known, of any infection from dirty pipes.

It is an admitted fact that the throat swarms with bacteria at all times, and possibly the Loeffler bacillus is amongst them occasionally,‡ but they are incapable of doing much harm to the individual carrying them if he be in good health. Through the depressing influences referred to, they are capable of multiplying and increasing in virulence, and finally causing infectious sore throats. The sore throat may be taken indeed, by the individual who first receives or develops

* Med. Record. Sept. 30, 1893, p. 422. † Ibid., Jan. 28, 1893.

‡ That Loeffler bacilli are present at times in considerable number in the mucous secretions of the tonsils and pharynx of children apparently in good health, is now clearly established. This important and suggestive fact we owe to the researches of Drs. Chappell and Park, (v. N. Y. Med. Record, April 14, 1894.

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the bacillus, or he may infect other persons without himself having given evidence of sore throat or other disease.

From what precedes, we should see clearly how important it is to keep up an excellent condition of general health in order to avoid contracting throat diseases. It is also evident that proper disinfecting washes or gargles for the mouth and throat, are very important to keep it in a condition where infective bacteria will not develop, or if present, will not take on virulent properties. These precautionary measures which are, or may be useful at all times, and particularly for children, are likewise specially desirable for the delicate ones, and in time of epidemic or exposure in any way to the sources or causes of infectious sore throats.

The aggravating effects upon the condition of the throat of effluvia from sewers and drains is then most probable. While the distinct connection between the two conditions cannot always be determined, yet in very many cases it is clearly demonstrated. Moreover, reasoning by analogy, it should be true, inasmuch as sewage emanations are known to aggravate the severity of all the exanthemata, erysipelas, hospital gangrene and puerperal fever (Rigby); and it would seem that all diseases are more or less affected by these effluvia. (Hygiene and Public Health, Buck, v. I, p. 585; quoted from Parkes.)

That many cases of tonsillitis are immediately caused by the air of sewers and fœcal emanations scarcely admits of reasonable doubt. The evidence in this direction is very convincing, and I am persuaded that I have seen these affections dependent to a great extent upon the polluted air of sewers, cesspools, and of the soil. We can properly explain the throat affection, it seems to me, in the same way we do the development of typhoid fever. "The morbific agent conveyed through the medium of the air, finds its way into the houses from cesspools improperly located, or from drain pipes imperfectly ventilated or badly trapped, or from impure soil beneath and surrounding the dwellings. With the doctrine that the disease is not in the micro-organism itself, but in its products, we may have floating matter which is not living and yet which is infective; so the gases of certain kinds of decomposition may be sources of contagion without our deciding whether or not they have been produced through the agency of low forms of life." (Report of the Board of Health of the State of New Jersey, 1893, p. 76.)

"In same cases, sore throats have been confined to a particular part of the house, especially exposed to the effluvia from badly trapped drains; and as the water supply was unexceptional, and other sources of disease could not be found, there could be no doubt as to the source of the infection."

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Even though we might be inclined to give a different interpretation to some facts of an analogous kind, yet what we can say is, that in many instances in which the bad sewerage has been partially, or more or less thoroughly remedied, the throat affections have notably lessened or disappeared almost entirely for a while. In this direction, we have the unbiased and careful testimony of many judicious observers. Thus Dr. Billington of New York writes in regard to the etiology of diphtheria: "The relation of cause to effect has been demonstrated by the fact that in some buildings which had come to be looked upon by me as diphtheria nests, there has been no recurrence of the disease for quite a number of years since the evils referred to, viz: foul cellars, neglected cesspools, bad plumbing, untrapped sinks, no air-shafts, were removed through the efforts of the Board of Health. It is probable, moreover, that it is in some degree at least, a result of the removal of these foci of the disease, that the mortality from diphtheria in the ward of which Dr. Billington was an out-door visiting physician, has become very much less.* The experience of Billington is corroborated by that of others. Thus "the persistent attacks of enteric fever (and also diphtheria?) which formerly occurred at Eastney Barracks in England were due to sewer-air being forced back by the tide, no traps or ventilating openings being supplied; since this was remedied and ventilation carried out, no case of fever has occurred."†

On the other hand, I have known diphtheria and follicular tonsillitis to occur, and not infrequently in houses where the closest investigation could show no defect whatever in the sewerage of the house, where the cellar was clean and dry, where ventilation was excellent, and where indeed, no discoverable and satisfactory cause of the kind referred to could account for the development of these diseases. At first thought, this fact would appear to support those observers who claim that insanitary conditions, due especially to defective plumbing and drainage, have very little to do in occasioning an outbreak of throat disease. This statement I can only admit in so far as to say that imperfect sewerage is only one of the efficient causes of diphtheria and other throat disease, and that numerous other causes of these effections may and do exist. These other causes, already referred to, are especially active, no doubt, in times of epidemic. In cases of diphtheria and other throat diseases due to endemic causes, it is quite clear at times that the bad sewerage of the house acts in one of two ways: First, in causing a condition of the throat upon which the

* Diphtheria and Croup, p. 22, Wm. Wood & Co, N. Y., 1889.

† Hygiene and Public Health, Stevenson and Murphy, vol. i, p. 11, London, 1892.

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specific bacillus of diphtheria, or other micro-organism, can graft itself more readily; second, in developing, purpetuating, or perhaps in originating, the specific bacillus or other microbe which is frequently present in the air of a certain house or locality.

Again, in estimating the influence of drainage in producing throat disease in certain houses of the rich, and even though no visible defect can always be discovered, it must be remembered that in view of the complicated and multiform appliances of modern plumbing, they "have seemed to multiply rather than to obviate the insidious dangers from noxious miasms." (Billington, p. 23.)

Be it observed also, that American houses, as a rule, have no other way of changing the air than by opening the windows. Water-closets oftentimes are found with no connection with the outer air, located in the middle of the house, and what ventilation there is, is into the rooms of the house.*

According to Jacobi, cases of diphtheria which are traceable to sewers are very frequent. (P. 34, Treatise on Diphtheria, Wm. Wood & Co., N. Y., 1880). Yet he shows how in the same house one may see cases of dysentery, typhoid fever and diphtheritic sore throats, all due apparently to a common poison.

This should be admitted, says Jacobi, unless we assign a triple character to the poison, or banishing the belief in specific influences, simply attribute the causation of these diseases to the lowered tone of the system, and after this manner affording to morbid influences an opportunity to exercise their power.

The following case in my personal experience has in this connection, some interest. I was called to see a case of diphtheria in the practice of Dr. Smith Ely, New York. In regard to this case, Dr. Ely in a letter dated March 12, 1894, reports as follows: "In a persistent search for a cause of diphtheria in the C. children, some of the casing enclosing the drain pipe in the water-closet being removed, a piece of the pipe below the trap was found to be broken out, so that of course, the sewer gas mingled freely with the air of the house. This state of things might have existed for a long time. There was another slight case of diphtheria in one of the other children at the same time. In September, 1891, the same child who had diphtheria so badly, had typhoid fever. There were other cases in the neighborhood, however."

As is so frequent in similar cases, the possibilities of causation in infectious diseases are rarely single, or always definitely separated from one another.

* Parke's Hygiene with Am. Supplement, Wood & Co., 1884, p. 526, (Appendix).

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In certain families, diphtheria and follicular tonsillitis show a tendency to spread even though the sanitary conditions are of the best.* The connection, moreover, between diphtheria and preceding throat affections is occasionally most marked. This is probably the most important link between various insanitary conditions and the diphtheria which sometimes breaks out amid them. In this sense, defective house sanitation is often a cause of diphtheria by producing a morbid condition of mucus membrane, which favores the development of the specific bacillus; though, in other instances, there is reason to believe that the specific bacillus might be present in the sewers of a district, and thence gain access to a house.†

Moreover, now "many are entertaining the view that different microphytes may produce the same disease, or that the same microphyte is subject to modification in virulence and its products, and that disease may be influenced by the quality, as well as by the quantity of the invading irritant." (Report of Board of Health of State of New Jersey, 1893, p. 82).

Once the sewers have become infected, they may perhaps continue to be so for an indefinite period. If then for any reason, the plumbing fixtures of a particular domicile are defective, it is readily appreciable how sewer air laden with disease germs, finds access to it. (v. Report of State Board of Health of New York, 1893, p. 516).

Observation and experience have further taught us that sewers if not properly constructed and carefully attended to, are more detrimental to health than if there were none at all. (Report of State Board of Health of New Jersey, 1893, p. 282).

On the other hand, it is certain that in many instances, diphtheria does not require for its development any of the ordinarily considered anti-hygienic conditions. So far as we can determine accurately, the growth of infectious element of the disease is not invariably favored by bad air or imperfect ventilation. Witness the facts reported by Jenner‡ who states that he has seen more cases of this disease in hospital than in private practice; and again as showing how little the untoward course of diphtheria is thus influenced, we note that whilst half of Jenner's hospital cases died, *more than half* of his private cases died.

These opinions of Jenner are in a measure corroborated by the results of a careful comparison of a large number of cases of diphtheria, reported in Massachusetts (Jacobi). Among the conclusions of the official report, we find that "The contamination of the atmos-

* Lectures on Fevers and Diphtheria, Jenner, N. Y., 1893.

† Bri. Med. Jour., p. 430, Feb. 24, 1894. ‡ Loc. cit., p. 523.

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phere of the bed-chamber by the emanations from sewers, requires further study."

In this connection, Dr. Arthur Newsholme, medical officer of Health for Brighton, England, states that diphtheria had increased in England latterly, and more particularly in urban districts.

"In this particular it differed greatly from other diseases of zymotic type, which had been much mitigated in severity by sanitary improvements."*

If this fact be admitted, it is additional proof perhaps that diphtheria depends for its spread on contagion direct from infected materials. The presence of sewers favors its continued existence. Closely inhabited localities do the same thing also, by furnishing ready means for communication of the disease. Thus, in most cities, it has become endemic. (v. Report of State Board of Health of New York, 1893, p. 516).

Elsewhere† in speaking of tonsillitis, I have said: "It is perhaps a less familiar fact that exposure to a vitiated atmosphere will occasion the development of tonsillitis in children. Septic causes of tonsillitis are also often met with, and I am constantly advised of the fact that defective drainage may give rise to recurrent attacks of tonsillitis in children."

This statement seems to receive corroboration from Osler‡ who writes, "bad hygienic surroundings appear to have a direct etiological connection with the disease." In so many instances, defective drainage has been found associated with outbreaks of follicular tonsillitis that sewer gas is regarded as a common exciting cause. Bernabei has also found that in many cases of acute angina, streptococci are discovered and that these cases are epidemic and contagious.

Rendu and Bulloche are even of the opinion that all acute anginas are of bacterial origin, and similar opinions are held of late by many other observers. (System of Diseases of the Ear, Throat and Nose, Burnett, vol. ii, p. 250–251).

In many of these cases according to Kingston Fox (Trans. of Med. Sc. of London, vol. ix, p. 255) and Browne, the differential diagnosis from acute tonsillitis due to cold or other causes is made by the fact that the septic cases are bilateral in the beginning.

No doubt, as I have observed frequently, some children are rendered more susceptible to septic causes by the presence at the same time of a rheumatic habit or a strumous constitution. Nevertheless,

* Br. Med. Jl., p. 430, Feb. 24, 1894.

† Cyclopedia of Diseases of Children, Keating, vol. ii, p. 332.

‡ Practice of Medicine, p. 332.

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whenever recurrent attacks of tonsillitis occur in a child, or among children in the same family, it becomes a duty to see to it that the basins, sinks, lavatories, drains and pipes shall be examined as carefully as possible, to discover if there be any defect in the plumbing through which sewer gas may gain entrance to the house. It is important again to observe that frequently no doubt what is recognized as sewer gas in our houses, is the result of decomposition which takes place in the house drains themselves, rather than the result of decomposition in the distant sewer forced into our houses through the connecting drain (Waring). It is doubtless a fact that in many instances, the gases formed within the drain of a house are far more hurtful to health than the gases which circulate in the main sewers. This is frequently explained by the fact that pipes and drains of the house have become coated over with slimy deposits which in time are considerable, and because many of these soil pipes are never properly ventilated by a proper circulation of air.

A considerable degree of doubt exists in the minds of same practitioners about the evil effects of sewer air on the throat. This arises probably from "the notorious fact that workers in sewers are remarkably free from disease, and particularly from affections of the throat." Indeed, in the numerous citations which I have referred to personally, I have found no case in which one of these men is said to have suffered from diphtheria or tonsillitis. In many of the cases of disease which have been reported, the illness may have been only a coincidence,* and in no manner connected with their profession. It may also be said in regard to these cases of immunity, that these men have become gradually acclimatized to their usual surroundings, and that the others who were unfavorably affected by the work in sewers, had abandoned it and taken up employment of a different kind.

Whenever cases of diphtheria, follicular tonsillitis, or erythematous sore throat occur in a house where the plumbing is found to be defective, we should be careful not to ascribe these diseases necessarily to this cause. In what precedes it has been shown that throat diseases may and often do occur where the drainage is bad; they also occur where the sewerage is in perfect condition; they likewise appear where there is no sewerage at all to account for the outbreak. If the sanitary appliances are defective, we are naturally inclined to hold them responsible for the throat disease. And yet as a fact, few of our city, or country houses, even of those owned by wealthy people, come up in their plumbing arrangements to the requirements of a careful sanitary engineer. Despite this fact, the throat condition of

* Osler, Practice of Medicine, p. 261.

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the majority of people remains tolerably good and the percentage of those who suffer from diphtheria or less severe disorders of the throat, is comparatively small. Again, there may be some cause of throat disease other than that of breathing sewer air, which has been overlooked, and yet ultimately is proven to be the occasion of the outbreak. In illustration of this statement, I would cite "an outbreak of diphtheria in the village of Upchurch, England, which was finally attributed to the dust from collections of refuse from London dustbins accumulated in the neighborhood, and when the utter absence of any other source of infection was affirmed.*

The chronic poisoning with sewer air, especially in its relations with diseases of the throat is as we have seen, a subject to which considerable thought has been given, and yet the conclusions arrived at are uncertain. Still it would seem to be fairly well established by repeated observations, that the breathing of sewer air for any considerable length of time, and even though largely diluted, will occasion many of the throat affections. The clinical experience of numerous physicians, the evidence of sanitarians, all point strongly to this fact. It is also proven that evil effects of breathing sewer gas on the throat depends not so much on the quantity as the quality of the gas. If the sewer air contains the specific germs of diphtheria, it may become the carrier of this disease. At another time when the sewer air contains no such specific germs, it will be the bearer only of follicular or erythematous tonsillitis.

It is probable that sewer-air may at times be inhaled quite a long period without developing any symptoms of throat disease. At a later period, when the sewer-air becomes laden with harmful germs, a throat affection of infectious type may occur with certainty. In children, when no specific disease germs are at work, the symptoms of chronic sewer-air poisoning are frequently those of catarrhal tonsillitis. In time of epidemic, the catarrhal tonsillitis is apt to assume a severe or malignant type,† and a simple sore throat may under the influence of sewer-air assume a very serious and aggravated form. In children particularly, the susceptibility to the influence of air polluted by sewage emanations, appears to be greatest. They easily become languid, and may suffer from sore throat connected or not with diarrhœa or some other manifest disturbance of the digestive tract.‡

People are more apt to suffer from the evil effects of sewer gas during the night than during the day when the body is undergoing

* The Journal of Laryngology, March, 1894, p. 124.

† Reference Hand-book of the Med. Sciences, vol. vi, p. 435.

‡ Bartley, Reference Hand-book of Med. Sciences, vol. vi, p. 436.

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active exercise, and when the rooms are better ventilated. This is the reason why we hear patients say so frequently that they have gone to bed perfectly well and have awakened during the night or in the early morning, suffering from sore throat. It is quite possible that the ordinary putrefactive bacteria when germinating in pent up sewage may be the cause of the catarrhal sore throats produced by sewer air.*

Finally, it must be obvious "that the effect oftentimes is practically the same whether the fermenting sewage is contained in sewers, or has soaked into the ground about the house. Both may pollute the air with the same gases and may produce like effects upon the throat. When furnace fires are used in the house, the gases from a sewer-soaked soil may be drawn into the rooms above, giving rise to all the injurious effects of sewer-air."† "Or the position of the air inlet of a furnace near a drain opening, or other source of contamination may allow foul air to pass into the heating chamber of the furnace and be distributed through the house," thus becoming the efficient cause of diphtheria, follicular tonsillitis, or other throat disease.

The following communication I am indebted for to the courtesy of Dr. A. Campbell White, House Physician at the Willard Parker Hospital, N. Y. Dr. White's report appears to me very interesting and instructive. Miss Graf, the nurse from St. Luke's Hospital, referred to by Dr. Campbell, was seen by me both during and subsequent to her attack of diphtheria.

"I believe the impression is or has been quite universal that the origin of epidemics or of individual cases of this disease could be traced to faulty plumbing or some other condition giving rise to escape of sewer gas. That such a condition can be the primary cause of diphtheria we are now quite safe in denying.

We are very positive in asserting that the Klebs-Löffler bacillus is the organism at the foundation of every case of diphtheria, and we can be almost equally positive in saying that this germ cannot be found in sewer gas.

If the drain pipes are in a moist condition, as they almost invariably are, it is justifiable to believe that even though these pipes may be loaded with the bacilli of diphtheria, the gases escaping will not carry the infection necessary to produce this disease.

We can take for an example, and I think it is a good one, the wards in our hospital. Here we have some fifty patients, every one of whom

* L. c. p. 436.

† Parkes' Hygiene with American Supplement, Wm. Wood & Co., 1884, p. 526 (Appendix).

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has the bacillus of Klebs and Löffler in the throat or nasal passages. We can compare, for sake of argument, these throats to the sewer pipes, and the air these patients are constantly exhaling to the sewer gases. Now the ward of course, in spite of good ventilation, is constantly filled with the expired air of patients suffering some of them from the very worst forms of diphtheria, yet we have never succeeded in obtaining any Klebs-Löffler bacilli in the air examined, nor have any of the attendants who live twelve hours out of twenty-four in these same wards contracted the disease.

We are very firmly convinced that to contract this disease direct contact is absolutely necessary. Therefore sewage (containing the diphtheria bacillus) must come in direct contact with the patient by contaminating the water or some other such manner in order to be held responsible for an attack of diphtheria.

Nevertheless I do not hesitate in saying that constant exposure to sewer gas is a very important element in determining the severity of an attack of diphtheria and that in many cases it is the most important factor in bringing on a fatal result.

At the annual meeting of the Public Health Association held in Baltimore in 1875, Dr. Noel, after citing many cases in his own practice to bear out his statement, said: "We do not say that this gas is the direct cause of diphtheria, croup, etc., in the sense of being the one and only efficient cause, but we do contend, and we defy criticism here, that in Baltimore City it has most assuredly been the one constant factor invariably present in the most malignant and well defined house epidemics." We can give an excellent example in our own recent experience. I refer to case of Miss Gräf from St. Luke's Hospital, and the other two nurses who attended the family in Pennsylvania, a family of seven or eight members, living in a house where the plumbing is exceptionally poor and, as the nurses testify, where there is a constant odor of sewer gas penetrating the entire place. Now every member of this family, ranging in ages from under 5 years to 27 years, contracted the disease and every one died after a comparatively short illness. This is a most frightful mortality. The three nurses who attended these cases also contracted the disease, but immediately came to our hospital for treatment. Not one of them died and only one developed into anything like the malignant form assumed undoubtedly by the cases in Pennsylvania. We can hardly attribute this difference entirely to treatment, nor can we say they did not have the same exposure, but I think we are justified in believing that this marked difference in mortality was more than a little influenced by the fact that the former cases were treated in a sewer-poisoned atmos-

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phere, while the more fortunate nurses were cared for in a clean, well ventilated ward in our hospital where there is no taint of sewer gas.

Another important point to remember is that we are now finding many people in the very best of health walking around with the Klebs-Löffler bacilli in their throats displaying no signs whatever of diphtheria. Some of these people later develope the disease, in others the Löffler disappear without having caused any trouble or inconvenience. The former have been exposed to cold, have developed a catarrhal inflammation of the respiratory passages, or in some such way the throat, previously immune, is rendered susceptible to the bacillus. Now such a person, with the necessary bacilli in his throat, breathing in from day to day the atmosphere laden with sewer gas is almost certain to develop diphtheria, and we can readily see how easy it is to say (not knowing that the patient had previously the bacilli in his throat) that such a person contracted his diphtheria from the gas escaping from a defective sewer pipe, while on the contrary if he had not already the bacilli in his throat he might have gone on indefinitely breathing this vitiated air without even developing the disease.

We have ample reason to believe that sewer gas attacks the weak spots of the patient. Dr. F. Gordon Morrill mentions this point in the Boston Medical and Surgical Journal of 1884. Among the diseases brought on by sewer gas in this way he mentions, besides those affecting the throat, rheumatism, gout, asthma, pneumonia and even neurasthenia.

The influence of sewer gas upon diseases of the throat can be summarized as follows: 1st. Given a patient with a so-called weak throat, subject to frequent attacks of quinsy, etc., expose him to sewer gas he will probably develop amygdalitis. 2d. Given a patient in good health with a throat containing Klebs-Löffler bacilli, expose him for any time to sewer gas and he will probably develop diphtheria, or 3d, given a mild case of diphtheria expose him to sewer-gas and the disease will assume a more malignant type.

Therefore, although sewer gas plays an important part in the etiology of some throat diseases, it can by no means be held responsible as the primary all efficient cause."

The following letter is also of considerable interest. I saw the last boy who died, in consultation with Dr. Chandler, and agreed entirely with him in regard to the diagnosis.

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So. Orange, May 26, 1894.

Dear Doctor: The history you desire is in brief as follows: In April, 1888, Mr. C. built and occupied a house on —— Ave., Orange, N. J. In April, 1890, his oldest son was very sick with diphtheria, but recovered. No unusual amount of sickness affected his family for two years later in this house. He then moved to another town.

A Mr. G. occupied the same house after Mr. C. vacated. He had a family of three children. All had been previously well. They soon sickened of various disorders of no especial name or magnitude. After living in the house about one year one child developed diphtheria and after a week's sickness died.* About a month later a third child was attacked with the same disease. By the advice of their physician they left the house on the third day of the child's sickness, but that child also died.

The plumbing and draining of the house was found to be defective, the wash tray in the butler's pantry was without a trap. Mr. C., who first lived in the house used the tray, but *always kept the plug in the outlet* and there was *no overflow*. Mr. G. did not use the tray and the plug was often out and a "bad odor" was noticed from the tray.

An outlet pipe, to remove surface water which might collect in the cellar, ran *directly* to the *cesspool* and was also *without a trap*. Thus there were two vents from the cesspool directly into the house.

These are, briefly, the facts without comment. They speak for themselves.

Very truly yours,

Wm. J. Chandler.

Dr. Victor C. Vaughan, of Ann Arbor.

If I should enter into a discussion of Dr. Abbott's paper, I should have to differ from his conclusion, but as I do not wish to criticise his paper without giving him an opportunity to reply, I hope that I may be excused from saying anything.

Dr. William H. Welch, of Baltimore.

As the discussion upon this subject has already exceeded the time allotted to it and another subject is to be presented this afternoon, I do not feel warranted in prolonging the discussion. I must express my appreciation of the valuable experimental work done by Dr.

* Another child was stricken with a light attack of the same character before the first child died, but recovered.

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Abbott and presented in his paper. In view of the vague opinions current upon the subject of sewer gas poisoning, it is a relief to obtain definite information on physical, chemical and biological conditions which must be taken into consideration in reaching positive conclusions upon this question. I was interested in Dr. Jacobi's statement that the study of a large series of cases of diphtheria in New York by the Health Board had not substantiated the prevalent view that there is a relation between diphtheria and emanations from sewers. This is important as diphtheria is that disease which has been believed by many to be especially referable to sewer gas poisoning, either as a primary or secondary cause.

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THE DISTRIBUTION OF LEPROSY IN NORTH AMERICA.

BY JAMES NEVINS HYDE, A.M., M.D.,

Professor of Diseases of the Skin, Rush Medical College, Chicago, Illinois.

In an attempt to discuss within the limits here set, the questions connected with the distribution of leprosy in the northern part of this hemisphere, it is expedient at the outset to simplify the terms involved. By the designation, lepra, it is here intended to refer exclusively to that complexus of symptoms to which the title has, up to within a relatively recent date, been restricted in the annals of medicine. Aggressive attempts have lately been made to identify as among the manifestations of leprosy, syringomyelia, Morvan's disease, scleroderma, morphœa, Raynaud's disease, ainhum, and some other affections. These it is averred, are symptoms of an attenuated form of the malady occurring in the races best fitted by inheritance and development to resist the graver incursions of the plague. These views, not being as yet established by sufficient evidence, may be for the present purpose dismissed from consideration.

But some at least of the time-honored distinctions established by authors in this field, may be now with safety set aside. The tubercular, anæsthetic, macular, mutilating, and mixed forms of lepra should figure no longer in the statistics of the disease. They are not varieties but different external expressions of one and the same malady, often simultaneously shown in a single patient.

The bacilli of lepra have traveled from their home in Asia to the American continent, eastward and westward in two distinct armies of invasion.

That advancing to the west divided into two companies, unequal in size, on the Syrian shores of the Mediterranean. Of these, the smaller division passed northward to attack Norway, Iceland, and Greenland; the vaster number straggled in an irregular line to the southward over Egypt, and southern and western Africa.

The northern germ colony, somewhat attenuated by reason, perhaps, of the vast wastes of cold seas traversed, ranged toward the northern boundaries of this hemisphere, along the shores of the St. Lawrence River. The southern colony, finding a more congenial soil and climate in Brazil and Guiana, multiplied and strengthened till their outposts had spread as far to the north as the West Indies and Mexico.

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A single line of invasion was observed from Asia to the western coastline of this country. It extended directly from China, through the Samoan and Hawaiian Islands, to the seaboard of North America.

The prevalence of leprosy on the Sandwich Islands, in consequence of their commercial proximity to this country, has attracted the attention of medical men and others since the year 1830, when it is believed the first authentic case was recognized. At the settlement in Molokai, there are at the present date 1142 lepers. As to the number at large, the health officers of Honolulu believe that it is not great as a consequence of a recent and carefully executed examination of the people in the several islands of this group.

Finally, through the port of the city of New York, with its innumerable maritime connections, a door has been open to the immigration of the germ-carriers of this disease, from every country of the globe.

In studying the distribution of leprosy in North America, after its introduction by the several routes thus recognized, several interesting facts are readily established: first, that with respect to the spread of the disease, the northern has suffered far less than the southern portion of the American hemisphere; second, that the prevalence of disease, even in this latter portion, is far greater in the southern than in the northern provinces; third, that, as compared with other continents afflicted with the disease, North America suffers to-day less than either Asia or Africa; and, lastly, that if Mexico and the other countries south of the border line of the United States be not considered in the comparison, the relative prevalence of the disease in British America and the United States scarcely differs from that of all Europe, if Norway be excluded.

With respect to the distribution of leprosy in British America, I have arranged with my colleague, Dr. J. E. Graham, of Toronto, Canada, to report fully upon this part of the field. It appears from the records that about two hundred and fifty lepers have been cared for in the province of New Brunswick, since the year 1815. Dr. Graham will show that there are probably not more than thirty lepers living to-day in that whole region.

The Consul-General of the United States in Havana, under date of the 20th of March of the present year, sends me a communication from Dr. Daniel M. Burgess, Sanitary Inspector of the Marine Hospital Service, reporting that more than eighty male and female lepers are inmates of the Leprosy Hospital of Havana, and that the prominent physicians of that city estimate that there are more than that number living at large in the town. Infected individuals are known to be living also in Puerto Principe, Santiago de Cuba, and in every

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town of considerable size on the island. A low estimate, therefore, would allot between 300 and 500 lepers to Cuba alone. As to the segregation of the victims of the disease, if there be any laws providing for the same, they are in that country a dead letter.

In the Lepers' Home of the island of Jamaica, there were, at the date of the last report, eighty-six subjects of the same disease, the total number historically registered being between 700 and 800. Reports from the island of Trinidad enumerate 212 lepers at date of last report (*loc. cit.*), the estimated number for the entire island being 481. In Barbadoes, there are between sixty and seventy; in Antigua, twenty-one; in Guadaloupe, the disease is reported to be "endemic." The exact number of cases in St. Kitts, Nevis, and St. Vincent, where it is known to prevail, is undetermined. As a rule, there is no isolation of lepers in the British West Indies.

Turning to the main coast, the average number of lepers in the Lazaretto of the City of Mexico is thirty. The disease, which is here known as the "Mal de San Lazaro," has existed in the country since the conquest by Cortes; and outside of the capital of the republic, and especially in the western regions it prevails, but to an extent not known. The Mexican authorities, however, believe that leprosy is at the present time not extending.

Numbers of lepers are seen in Panama by travelers in that country; but here, as in some other of the States of the southern portion of North America, statistical figures are not accessible. The disease is sparsely scattered in the mountain regions of Costa Rica, and also in Nicaragua, Honduras, and Salvador. The same is true of Guatemala, but in that country attempts have been lately made to ensure strict segregation of the infected.

The United States of America has thus been exposed to incursions of leprosy from each of the four principal points of the meridian—from the north, from the south, from the east, and from the west. That the total result up to this date has been relatively insignificant, has been due, not to a wisely directed vigilance, nor to barriers erected by science. It has been owing rather to the accident of the general excellence of the habits, of the resources, and of the environment of our people.

At this date, the distribution of lepers in the United States cannot be determined with accuracy, either for the present or for the past. Some of the reasons for this unfortunate fact are these:

1. The United States Government has neglected its obvious duty in this matter. It is admitted by the authorities that the figures representing the number of lepers in the country, collated at the date of

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the last census, have not yet been compiled; that it is doubtful whether they ever will be compiled; and that, even if so compiled, the result would be so incomplete and unsatisfactory as to possess but little value.

2. The authorities in but a few States have taken the needful steps in the proper direction.

The State Boards of Health of Maine, Pennsylvania, and California, require the reporting to them of all cases of the disease. Segregation or isolation of lepers is required by law in Pennsylvania, New York, and Wisconsin only. In Pennsylvania and California, the county authorities provide for the maintenance and medical care of lepers. In Minnesota the infected are registered and kept under observation, but not segregated. These local and insufficient attempts to regulate a disease threatening the population of forty-nine States and Territories of the Union, illustrate well the uncertainty that exists respecting its geographical distribution within our borders.

3. In the absence of a trustworthy official collation of facts, it is well known that the families of lepers here, as in other countries, as a consequence of fear or shame, or for other reasons, often wilfully conceal, so far as they are able to do so, the existence of leprosy in one or more of their number.

4. Many of the general practitioners of the United States are so unfamiliar with leprosy they are unable to recognize it when confronted with it. This is not set down as a reproach, but as a fact based upon their own admissions, and an experience in recognized cases. This unfamiliarity with the disease, due to the infrequency of its occurrence in practice, has an unfortunate issue in two directions as respects the clouding of the question under consideration. It involves, first, the occasional non-recognition of the infected leper; it also at times, results in affixing the stigma of that disease upon patients with formidable evidences of a malady due to some simpler cause.

5. Among the cases of leprosy reported by wholly competent men as existing in different parts of the country it is often difficult to determine whether one or more patients do not also appear in other reports, the figures being thus duplicated to confusion. For many reasons it is often unwise and cruel to identify lepers by their individual names in reports of this kind.

6. The chances of embodying a single case in two or more separate reports of competent physicians is greatly enhanced by the movement of some leprous patients from one part of the country to another. In the Orient the leper is usually a pauper—a deformed and repul-

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sive object of charity by the roadside. The United States, however, enjoys the unenviable distinction of being the one country where some of its lepers have the ability and opportunity to travel from one point to another—at times in a Pullman car, and often with a liberally provided purse. The following travels of lepers in and across the Union are cited merely as illustrations of this fact; all are substantiated by well-known details: Six trips from New York to San Francisco; seven from San Francisco to New York; one from San Francisco to Chicago; three from Chicago to New York; one from Nebraska to Illinois; one from Florida to Illinois; one from Chicago to St. Louis; one from New York to Florida. One of the Chicago lepers decided to visit for awhile his home in Sweden. He left the American city, crossed the ocean, passed through Stockholm on his way to his native village, retraced his route, and finally returned to Chicago, where he died one year later of his disease.

The credit of first collecting statistics as to the number and location of lepers in the entire country is due to the American Dermatological Association. Long before the subject had become the favorite theme of the sensation-monger upon the lecture platform, in the magazines, in the daily press, and even in the pulpit, this Association under the painstaking leadership of its first President and first chairman of its Statistical Committee, Dr. James C. White, of Boston, began its investigation of this field. In the year 1878, a twelvemonth after the date of its organization, it published its first *Report of the Statistics of Cutaneous Diseases in America*, and in this report were included three new cases of leprosy, with detailed returns of the number of cases of lepra in Maryland, South Carolina, Georgia, California, Minnesota, New Brunswick, and in the Hawaiian (Sandwich) Islands. Since then the Association has collated a total of 204,860 cases of skin disease, including 56 cases of lepra, the percentage being 0.022. These figures do not, of course, represent the number of lepers in the United States, but only the number under observation of the experts represented in the membership of this body, or those reported directly to them.

Beginning, then, with these figures as a basis, there can be added to them the observations made by intelligent members of the profession connected with State Boards and Health Departments, as well as those of trained physicians, competent by education and experience to establish a correct diagnosis of leprosy. Most of their names appear in the bibliography appended to this report, which includes a list of more than one hundred and twenty papers, for the most part touching on leprosy in America, and contributed by American authors.

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It is scarcely needful to add that in the compilation of reported cases it is proper to give large credit to the work done by such well-known American writers on the subject as Dr. H. S. Orme, of San Francisco; Dr. C. Grönvold, of Minnesota; Dr. N. W. Blanc, of New Orleans; Dr. Benjamin Lee, of Philadelphia; Drs. Allen, Morrow, and Piffard, of the city of New York; and Dr. W. H. Geddings, of South Carolina.

It may thus be approximately determined that the cases of leprosy heretofore recognized in the United States have been distributed as follows: In Arkansas, 3; in California, 158; in Dakota, 2; in Florida, 6; in Georgia, 1; in Idaho, 2; in Illinois, 13; in Indiana, 2; in Iowa, 20; in Louisiana, 83; in Maryland, 4; in Massachusetts, 5; in Minnesota, 120; in Missouri, 2; in Mississippi, 2; in New York, 100; in New Jersey, 1; in Oregon Territory, 3; in Pennsylvania, 6; in Utah, 3; in Wisconsin, 20—a total of 560.

Thus much by way of an approximate estimate of the number of lepers which have been at one time or another upon the soil of the United States. In attempting to indicate upon a map the proportion and distribution of these cases, by placing dark spaces within the outlines of those States respectively where they have been observed the size of the spaces in each being proportioned to the number of cases—it becomes evident at a glance that the geographical proximity to a colony of lepers, of any point in this country where the population is relatively small, is of less importance as respects an accumulation of cases than is commercial activity between widely distant populous centres whose intercourse is by maritime traffic. Thus the southernmost point of Florida, which has been for at least half a century nearest a large leper colony, has harbored far fewer cases than either San Francisco or New York. The numbers represented in a group of the Northwestern States, illustrate the tide of settlement of Scandinavians deflected around the great lakes in their transit from New York to a country and climate approximated to that of the Swedish and Norwegian peninsula.

It is a much more difficult task to form even an approximate estimate of the number of lepers living to-day on the soil of this country. Under date of the 13th of March of the present year, Dr. H. R. Brown of the Health Department of the city of San Francisco, reports that there are but eleven lepers in the pest-house of that city: two from Hawaii, one from Finland, one from Mexico, one from Madras, and six from China. Dr. J. R. Laine, of the California State Board of Health, supposes that there are perhaps fifteen others in the entire State. This would give a total of twenty-six cases in Cali-

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fornia; and, in the view of the same authority, there has been no increase of the disease there in recent years. The proportion of these cases to those altogether reported from that State, 158 in all, may be, for the purpose of approximate estimate, observed from some of the other figures collated. In this way it may be computed that there should be ninety-two lepers now alive in the country. There are actually two lepers in Florida; and usually from one to three in Illinois, and the same number in New York. One is known to be in Texas, two in Idaho, two in Pennsylvania, four in South Carolina. There are probably more than threescore of lepers in Louisiana, and less than half that number in the States of the Northwest named above. This suggests an approximate estimate of between one hundred and two hundred cases of leprosy. Competent observers have estimated that the total number is between ninety and one hundred and fifty. It can scarcely be doubted that in each of the last fifteen years there have rarely been fewer than one hundred lepers living within the territorial borders of our country.

These figures are to be viewed neither with the hysterical alarm of the timid nor with the careless indifference of the unwary. They call for intelligent discussion and for a wise provision, alike for the need of the unfortunate leper and for the safety of the community in which the infected are now living. There are greater problems presented to the physicians of America than those growing out of these interesting statistics. Tuberculosis alone exposes at present, to a greater danger than lepra, the health of our population. But enough lepers are now sheltered upon this soil to offer a menace to the welfare of its inhabitants, and to propose a problem in State and sanitary science, which I believe the General Government alone can rightly solve.

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THE DISTRIBUTION OF LEPROSY IN THE DOMINION OF CANADA.

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Leprosy exists in the Dominion of Canada in three localities in the counties of Gloucester and Northumberland, Province of New Brunswick, in Cape Breton, Nova Scotia, and in British Columbia.

In New Brunswick the counties mentioned extend along the south shore of the Bay of Chaleurs and the east shore of the Gulf of Newfoundland.

The leper hospital is situated in Tracadie, the parish for many years principally affected. The inhabitants in the affected parishes are nearly altogether of French origin, some having come from Acadia and others from neighboring parts of the Province of Quebec.

Leprosy has been known to exist in this district since 1815. In 1844 a hospital was erected on Sheldrake island, and in 1849 the present lazaretto was built.

I have not been able to find out the exact number of leprous persons who have lived in this district, but one hundred and sixty-seven deaths are recorded. If to these is added the number of patients at present in the hospital, twenty-one, and allowance is made for imperfect registration probably the whole number of victims would amount to somewhat over two hundred.

The smallest number of patients at any time in the hospital (in 1876) was sixteen, six males and ten females. The greatest number at any time was thirty-seven. According to the report made by Dr. A. C. Smith to the Minister of Agriculture for 1893, there were twenty patients in the hospital, eleven males and nine females, of these eleven were in the early stage, six in the second, and three in the advanced stage. During that year six deaths had occurred and four new cases were admitted.

From a letter dated May 9, 1894, I find that there were then twenty-one patients in the hospital of whom sixteen suffered from the tubercular and five from the anæsthetic variety.

In the report already referred to Dr. Smith says : " I find that here as in other countries poor sanitary surroundings, defective and bad dietetic conditions, and uncleanliness are important factors in the spread of leprosy.

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The patients, as shown by many of the reports, are subject to typhoid fever, rheumatism and especially erysipelas and frequently succumb to these intercurrent diseases. The great prevalence of erysipelas Dr. Smith considers to be due to the condition of the hospital, an old building situated in a poor locality.

In the early history of the lazaretto patients were compelled by law to enter, but the difficulty experienced in carrying out this regulation was so great that it was repealed. Patients are not now compelled to enter the hospital nor are they forcibly detained after they enter.

The segregation of the lepers is fairly well accomplished. The priest of the parish working in harmony with the physician and the sisters of the lazaretto seek out the affected individuals and persuade them to enter the hospital. It cannot be said, however, that the isolation is at all complete.

In looking over Dr. Smith's reports for the last ten years one notices the varying number of patients in the hospital. When the number is small the attending physician indulges the hope that the disease will soon die out, and perhaps in the following year a new focus is discovered and as many as five or six new patients are admitted.

In the last report Dr. Smith states that while on a tour of inspection during 1892, he found leprosy cropping out in unexpected places, and "he fears that for years to come new cases will be met with and that only by permanent detention and isolation can it be kept checked or stamped out."

In the report dated Dec. 31, 1888, the following interesting case is given: "A boy, now fourteen years of age, was, when three years old, attended by a leprous woman during the healing stage of a burn. Leprosy afterwards made its appearance and he is now in an advanced stage of the disease. His parents, brothers and sisters are free from it."

In the last report the following is noticed: "I have made many excursions into the infected districts during the year, and now have two or three cases remaining under supervision which I do not consider it advisable to bring into the lazaretto at present. For reasons given above, over-crowding might end in disaster.

From this it would appear that the hospital accommodation is quite inadequate for the proper care of the leprous patients. It is to be hoped that such a state of affairs will not long continue.

In the September number of the Canadian Journal of Medical Science, a paper appeared written by Dr. McPhedran, which reported the investigations made by the late Mr. Fletcher, a medical undergraduate, into the disease as he found it in Cape Breton. So far as I

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am aware this is the first published account of the existence of leprosy in that locality. Dr. Smith afterwards visited Cape Breton, and under his advice the patients were removed to the Tracadie Hospital.

In the report for 1891 the following statement is made: "Leprosy has practically died out in Cape Breton. I may, however, add that I have recently found two persons developing suspicious symptoms whom I am watching." In a letter which I recently received Dr. Smith remarks that leprosy is almost a thing of the past in Cape Breton.

Almost eighteen months ago I wrote to Dr. Milne, then Medical Health Officer of the city of Victoria, asking for information as to the existence of leprosy in British Columbia.

In a letter dated Nov. 12, 1892, Dr. Milne reports as follows:

In British Columbia there are seven cases of leprosy, all Chinese. The disease was never known to exist in this province until two years ago when it was discovered among the Chinese. The lepers are kept on Dorsey Island, situated to the east of the city of Victoria, and a little to the north of the straits of San Juan de Fuca. The disease in all the cases has developed within the last few years and the patients have now been kept in quarantine two years. One of the patients came from New York City, where he had worked in a laundry for many years. While passing through British Columbia on his way from New York to China the disease was discovered. The steamboat authorities refused to take him as a passenger and the province was thus forced to look after him.

There are no other inhabitants on Dorsey Island where the lepers live by themselves, and seem fairly contented. The majority suffer from the tubercular form. The colony is now partly supported by the City of Victoria and partly by the Dominion Government.

In a letter which I recently received dated April 17, 1894, Dr. Milne reports one death and one new case, a white man, the first instance in British Columbia of the extension of the disease beyond the Chinese race. He further states in regard to the leprous colony: The patients are contented on the island where they are engaged in farming. They raise such of their own provisions as vegetables, poultry, etc., and are well cared for by the authorities, so far as their food and clothing are concerned. They have, however, no one to look after them and they do not make any attempt to escape, although the islands are not far apart.

In conclusion, the total number of lepers in Canada is probably about 32, distributed follows: In the hospital at Tracadie, 21; cases in the surrounding district not yet admitted, 2; in Cape Breton (per-

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haps), 2 : in British Columbia, 7. I do not know of any sporadic cases outside of the districts mentioned.

Two points are to be noticed in connection with the distribution. (1) The disease exists altogether on the sea coast, partly for the reason that the affected individuals after landing remained near the coast, and formed a focus of disease. (2) The disease is and has almost always been found to affect foreigners. There have been a few instances of infected individuals of the Anglo-Saxon race, but the number is very small.

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THE DIAGNOSTIC FEATURES AND TREATMENT OF LEPROSY.

PRINCE A. MORROW, A.M., M.D.

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The clinical picture of leprosy drawn by many of our text-book writers, is that of a disease as readily recognizable by its typical features as it is repulsive by its hideous deformity. This common conception is derived from examples or representations of the disease in its fully developed or final stage. With its initial manifestations and the varied phenomena exhibited in the earlier stage of its evolution, few medical men in this country are familiar. While it is undoubtedly true that the clinical features of a case of leprosy typical in its development and advanced in its evolution are so striking and characteristic as to be absolutely pathognomonic, it is equally true that in its earlier stages, and even in fully developed cases with atypical manifestations, there is no disease in the entire domain of pathology more difficult of recognition. It is precisely this latter class of cases which have a most important relation to the general subject now under discussion. Leprosy in this country is essentially of exotic origin, and Dr. Hyde has told you that the vast majority of lepers in North America are imported cases. Dr. White will doubtless urge the establishment of a rigid quarantine against the immigration of lepers, as the most effective prophylactic measure. In the writer's opinion, however, no system of quarantine can be instituted, sufficiently searching and rigid, to exclude a disease so little manifest on ordinary examination as leprosy.

Mr. Jonathan Hutchinson states that on the occasion of a visit to the Leper Hospital in Norway he examined a number of the inmates who appeared to him perfectly well. Asking to be shown the means by which identification was arrived at, little brown patches, sometimes a single patch, on the limbs, were pointed out which were anæsthetic in their centers. Referring to the efforts then being made by the U. S. authorities to prevent the ingress of lepers by the inspection of all Norwegian immigrants before they were allowed to land, the Bergen physician said "they only recognize bad forms and these but seldom go over; they don't know such cases as these."

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It will be my object in this paper to consider more especially the diagnostic signs of the disease at an early stage when they are so little manifest on ordinary examination as to escape identification.

The difficulties which attend the diagnosis of leprosy depend upon its prolonged period of incubation, the absence of any initial lesion that might connect it with a known exposure, the indeterminate character of its prodromal symptoms, and, finally, the multiplicity and banality of its manifestations. The fact that leprosy is essentially a proteiform malady is not sufficiently appreciated. While syphilis may surpass it in the number and variety of its eruptive elements, the cutaneous manifestations of leprosy most accurately imitate many of the ordinary dermatoses. This imitation is carried into the realm of neuro-pathology: nerve leprosy simulating most deceptively the manifold forms of neuritis, of toxic, traumatic, and constitutional origin.

The earlier manifestations of leprosy, unlike those of syphilis, are in no sense "specific" or peculiar to the leprous process; there is nothing regular in their mode of evolution, nothing constant in their appearance, nothing distinctive in their morphological characters, they are so variable, uncharacteristic, and absolutely indefinite that they would never be ascribed to leprosy in any country where the disease was not endemic or there were not decided reasons for suspecting its presence.

As is well known, the lesions of leprosy chiefly affect the skin and nerves, and according to the determination of the morbid process towards the cutaneous or nervous system, two principal forms are recognized. The prodromal symptoms of the *tubercular form* possess but little diagnostic value; they may be so slight as to escape recognition. The malaise, general debility, vertigo, digestive disorders, fever, etc., are common to other diseases. The febrile movements, formerly ascribed to cold, damp, or malaria, we now recognize as due to the invasion of new parts of the body by the bacilli, and the toxic effects produced by their emanations.

The prodromal symptoms of the *anæsthetic form* are much more variable, and quite as uncharacteristic. They consist chiefly of disorders of sensation, hyperæsthesia, formication, pruritus, and sensations of burning, tingling and numbness. The pain and motor weakness often present are commonly ascribed to rheumatism or neuralgia. In both forms symptoms referable to changes in the mucous membranes of the upper air passages, slight hoarseness, rhinitis, epistaxis, etc., not infrequently precede the outbreak of any eruptive accident.

The first objective signs of leprosy are usually manifest in the form of erythematous spots or patches of variable size, shape and color.

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These spots almost invariably make their first appearance upon exposed parts, the face, hands and feet; later they become more generalized. The exanthem sometimes resembles that of the eruptive fevers. I have recently seen a case in which the initial rash had been diagnosticated as measles from which it was differentiated by its persistence. For three years it faded and reappeared a number of times in the form of erythemato-papular lesions, before the characteristic tuberculation took place. Still another in which the right side of the forehead and cheek was occupied by a slightly raised erythematous patches of a sombre-red color, simulating perfectly lupus erythematosus.

In the tubercular form the spots may come and go a number of times before remaining permanent or becoming the seat of tubercles. They are to be differentiated from the lesions of exudative erythema and the macular syphilide, by their localization and their slower involutive changes.

The erythematous spots of the anæsthetic form are characterized by their permanence, their tendency to clear in the centre while spreading peripherally, their achromic changes, and later, by their anæsthetic centres. They are to be distinguished from the patches of chromophytosis, morphœa and vitiligo.

Months or years may intervene between the first appearance of leprous erythema and the development of tubercles. The tubercles which constitute the most specific sign of this form may develop upon the erythematous patches or upon previously clear surfaces. They vary in form, color, consistence, and in size from that of a pin-head to that of a small nut. The tubercles may remain stationary or progressively enlarge. After full maturity they may undergo spontaneous resorption and reappear a number of times. They may soften and break down forming shallow or deeper ulcers or they may undergo a fibrous induration into small hard masses which persist indefinitely. They may be discrete or aggregated in patches. Their seats of predilection are the facial mask and the extremities, but they are not confined to these regions. Situated along the external malleolar region and the front of the leg they bear a most deceptive resemblance to the nodules of erythema nodosum. When small they have been mistaken for the papules of lichen planus, acne indurata, rosacea and sycosis. When larger the tubercles may resemble the tumors of molluscum fibrosum, sarcoma, carcinoma, mycosis fungoides, syphilis, and lupus vulgaris.

The diseases with which leprosy is most apt to be confounded are syphilis and cutaneous tuberculosis with which it shares the patholog-

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ical peculiarities of cell infiltration of the connective tissues followed by disintegration of the morbid products and ulceration.

The lenticular tubercles of leprosy resemble absolutely the small papular syphiloderm. The tubercular syphilide which bears the most deceptive resemblance to leprosy is distinguished by the more rounded contour of the lesions, their development in circular or crescentic forms, their absence from the lobes of the ears and their more general distribution. The ulcers of syphilis are more rounded, more superficial, and more circumscribed in extent. The ulcerations of leprosy do not present a serpiginous mode of extension. Lupus is distinguished by its occurrence in the form of isolated patches, and its more limited localization. In all doubtful cases of tubercular leprosy, the demonstrable presence of the bacilli in the tissues or liquid exudates establishes the diagnosis.

An early manifestation of the anæsthetic form is a bulbous eruption which may even precede the erythema. It is to be distinguished from pemphigus vulgaris. Frequently the first indication of the disease, especially in children, is the existence of burns, scalds or other traumatisms which the patient has received unconsciously. In tropical countries where patients go barefoot the so-called plantar ulcer is often an early sign.

The phenomena of nerve leprosy are essentially those of multiple neuritis ; they consist of hypertrophy and nodular infiltrations of the nerve cords, readily recognizable in the ulnar and peroneal nerves, paralyses of special nerves, with atrophic and degenerative changes of the muscles, giving rise to contractures and the peculiar "bird claw" deformation of the hands. Anaesthetic leprosy has been mistaken for progressive muscular atrophy, arthritis deformans, hysterical paralysis, mutilating scleroderma, Raynaud's disease, syringomelia, etc. The lesions of the bones and joints with mutilations and deformities which commonly occur in the advanced stage can scarcely be confounded with the clinical picture of any other disease. As the bacillus cannot be demonstrated in the lesions of nerve leprosy, in this form anæsthesia constitutes the most important diagnostic feature.

Since leprosy is exclusively human in its origin the history of known contact with a leper, or of residence in a leprous country, is of great diagnostic worth.

In this necessarily brief sketch I have been compelled to pass over the leprous manifestations of the mouth, nose and throat, the ocular lesions, the changes in the cutaneous glands, the alopecia, the visceral complications, and other pathological coincidences which furnish important diagnostic indications. Want of time also forbids more than

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a passing allusion to those *atypical* forms of the disease in which a deviation from the normal plan of evolution introduces an element of confusion. While as a rule the lesions of leprosy are more or less symmetrical, cases are met with in which the manifestations are strictly unilateral—I have had under observation a case in which twelve years after infection the only symptoms were dystrophia and anæsthesia of the right hand, forearm and ankle; another of ten years' duration in which the manifestations were limited to three or four erythematous patches with anæsthetic centres on the left side; still another in which the sole signs of the disease were a single tubercle on right cheek and anæsthetic changes in right hand.

Treatment.—It would be presumptuous in a medical man living in a country where leprosy is not endemic, to formulate conclusions as to its treatment, based upon his own necessarily limited experience. For results possessing definite value we must rely upon the clinical experience of competent medical men living in leprous countries, who have had opportunities of testing remedies on a large scale and during a long period. Unfortunately this clinical testimony, instead of being conclusive, is of the most conflicting and contradictory character. In estimating the value of treatment, certain possible sources of error should be considered. The study of the natural history of leprosy, abandoned to its own evolution and uninfluenced by treatment, shows that it does not pursue a progressive, uninterrupted course,—the morbid process alternates between activity and repose,—it often presents remissions sufficiently prolonged and complete to give deceptive indications of a cure. It may be formulated as a general law that recessions are the rule in leprosy, especially in the earlier stages. If a remedy happens to be given when such a remission is about to occur, the observer is apt to attribute the spontaneous subsidence of symptoms to the treatment employed. On the other hand, if the treatment is instituted coincidently with an exacerbation or new outbreak, it is condemned as a failure. In the writer's opinion the claims of most of the remedies which have been vaunted as "specifics" in leprosy are largely based upon *post hoc* conclusions as to results, which, rightly interpreted, are mere coincidences.

Without passing in review the vast number of drugs and methods of treatment which have been employed, we will glance at the clinical testimony in regard to a few which at the present day are most in repute.

Chaulmoogra oil perhaps ranks highest in professional esteem. It is claimed by Beaven Rake and most European observers that under its prolonged use discolorations clear up, leprous nodules undergo

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involution, ulcers heal, and there is a notable amelioration of all symptoms, while Bidenkap, Daniellsen and other experimenters have been disappointed with its results. Of Gurjun oil, which enjoys such a high repute in India, Hillis states that it is a most valuable agent in all forms of leprosy, exercising a specific action upon the sweat glands, evidenced by increased perspiration and return of sensation in the anæsthetic areas. This opinion is concurred in by many of the civil surgeons in India. On the other hand, Drs. Beaven Rake, Vandyke Carter and others depreciate its value, declaring that the results are disappointing. Salol so highly extolled by Lutz in arresting the leprous fever, causing the eruption to disappear and the ulcers to heal, is as strongly condemned by Dr. Cook, Superintendent of the Leper Hospital in Madras who, after an extensive experience, declares "Salol is, in my opinion, of no therapeutic value in leprosy, in fact, a decided failure." So with the entire list, salicylic acid, carbolic acid, creosote, crude petroleum, the sulpho ichthyolate of ammonium, arsenic, etc., which are highly praised by some observers, by others, equally competent, are condemned as useless or harmful.

We thus perceive that among men who have had the largest opportunities for experiment, there is a most woeful lack of unanimity as to the value of any of these various remedies.

In this country and Europe the therapeutic problem is complicated by the modifying influence of climate upon the course of the disease. It is a matter of observation that most lepers who come to this country get better for a time at least, with or without medication, and when treatment is employed it is difficult to differentiate between its effect and that of climate and improved modes of living. Thus in a case of pronounced tubercular leprosy referred to me some two years ago by a colleague in New York, the cutaneous manifestations entirely disappeared within a few months under the influence of large doses of Chaulmoogra oil internally with daily baths and inunctions of Gurjun oil. In another patient referred to me five years ago by Dr. Besnier of Paris there has been an apparent cure from the use of phosphide of zinc and strychnia. It is difficult to determine what precise measure of curative influence should be ascribed to the drugs in these cases, since in another case under observation for two years there was an arrest of the symptoms under the influence of hot baths, massage and general hygienic measures—no drugs whatever having been administered.

For purposes of conciseness the writer's views may be summarized in the following conclusions:

1. From the standpoint of scientific therapeusis a clear conception

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of the pathogenesis and pathological anatomy of leprosy is an essential condition in formulating the principles of rational treatment.

2. It is now generally conceded that Hansen's bacillus is the active, efficient cause of leprosy, and that the presence of the bacilli in the tissues sets up either directly, or indirectly through their toxines, the vast array of organic changes and functional disorders peculiar to the disease.

3. There is no substance known to science which introduced into the body is capable of destroying the bacilli without destroying the living cells which contain them.

4. Furthermore, from the nature of the pathological changes and the position of the bacilli in the deeper tissues, it is evident that no germicidal agent can be brought into direct contact with the pathogenetic organism, and hence, all treatment which has for its object the destruction of the bacilli is impossible of application.

5. The treatment of leprosy by injections of tuberculin has been disappointing in its results. Experiment has shown that the action of tuberculin is positively pernicious in setting free the bacilli in the tissues and determining the development of new foci of the disease.

6. The treatment of leprosy is essentially empirical; whether, as has been claimed, certain remedies act by virtue of their sterilizing properties upon the living tissues, rendering them unsuitable to the growth and multiplication of the bacilli, cannot be determined.

7. The more or less rapid development of leprosy depends upon the resistance of the tissues to the inroads of the bacilli. In exceptional but well authenticated cases this capacity of resistance is sufficient to dominate and destroy the pathogenetic microbes as shown by the observation of abortive cases in which indubitable signs of the disease definitely disappear and never recur.

8. This capacity of resistance may be strengthened by change of climate, improved habits of living and measures calculated to build up and maintain the general health at the highest standard.

9. Observation shows that the removal of a leper from an infected district to a more favored climate exerts a marked modification upon the course of the disease; there is, for a time at least, an arrest or retrogression of the symptoms. This lull in the manifestations is as a rule, disappointing in its duration. Of the 160 Norwegian lepers who have emigrated to this country, there is no record of a single definite cure.

10. A dry, moderately cool, mountain atmosphere is most favorable in its influence upon the disease. A hot, moist climate, or a damp, cold climate are both unfavorable.

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11. A nutritious diet of fresh meat and vegetables, warm clothing, exercise in the open air, freedom from exposure to damp and cold are important elements in the hygienic course of treatment.

12. The care of the skin by frequent hot baths, massage, with inunctions of oils, etc., should receive as much attention as the constitutional treatment.

13. The special remedies which clinical experience would indicate to be of most value are Chaulmoogra oil, Gurjun oil, arsenic and certain agents of the strychnos family; all are, however, more or less disappointing in their results.

14. All observers agree that in advanced cases where a general dissemination of the bacilli has taken place, curative treatment is absolutely futile. The most favorable conditions are that treatment be instituted early, and that it be prosecuted actively and energetically during a prolonged period.

15. The surgical treatment of leprous sores, necrosed bones, perforating ulcers, the excision of tubercles, amputation of the members, tracheotomy, various delicate operations about the eye, nerve stretching for the relief of pain, the removal of threatening complications, is of the most signal benefit.

16. Finally we may conclude that while medical science holds out no definite promise of cure to the leper, its resources are sufficient to arrest or retard the progress of his disease, to promote his comfort, and to prolong his life.

Dr. Arthur Van Harlingen, of Philadelphia.

The range of the discussion is of such a general character that the question of diagnosis and treatment need demand very little attention, at the same time I should like to say for the benefit of those who believe that all cases of leprosy that come under the observation of physicians, are diagnosticated, that I have seen a paper from a Norwegian authority stating that a large number of Norwegian lepers have passed through the United States although some sort of an examination was supposed to have been made. This was because the early symtoms of the disease were not recognized. It is the early cases that require the greatest care to distinguish between leprosy and other affections of the skin. The ordinary erythemata and some of the vegetable parasitic diseases of the skin are very apt to be mistaken for the earlier lesions of leprosy. Then the nerve lesions are often

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mistaken for other affections. It is impossible to say anything on these points in the very brief time which has been allowed me. The only thing that I can do is to urge most strongly that all physicians should prepare themselves and have a knowledge of these earlier lesions which it is necessary to be prepared to meet.

With regard to the treatment of leprosy there is nothing more satisfactory than segregation with the best hygienic conditions and the ordinary treatment used for all diseases of this same nature.

If a plan could be carried out for a National Home where the patients could be placed under the best hygienic surroundings, and those in better circumstances, be placed under surveillance under the care of friends, the treatment could be determined better than in the ordinary routine.

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THE CONTAGIOUSNESS AND CONTROL OF LEPROSY.

BY JAMES C. WHITE, M.D.,

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It should be wholly unnecessary, but I feel that it is due to myself to state, that it is of course impossible to present in any proper form the important subjects assigned to me in the inadequate time allotted me. I could in a fraction of it make the categorical statement that leprosy is a contagious disease, and that it has never been, and never can be controlled except by national laws of isolation, but the only and proper reply by those of a contrary opinion to such a presentation would be a denial as simple and categorical. Yet I can scarcely do more than this in my fifteen minutes. You see, therefore, the impossibility of making more than the briefest mention of the evidence upon which my opinions are based. I can only present in the baldest way the text for the discussion which is invited.

It will be well to consider first what should constitute the proof of the contagiousness of a disease.

This should be based upon the following points:

1. The history of its action upon communities and nations.
2. The study of its action upon individuals.
3. Its resemblance to other affections generally regarded as contagious.
4. The establishment of a satisfactory cause of such contagion.
5. The influence of control upon its history and course.

Let us apply these test questions to the disease we are considering.

1. The history of its action upon communities and nations.

You know as well as I the history of its course in mediæval Europe. Introduced from the East, the boundless home of so many scourges, it became a pestilence of so overwhelming proportions, that no influences of heredity alone could possibly account for its spread. Many thousands of lazarettos were erected to care for it, and the sternest laws enacted to control it. Belief in its contagious nature was then universal. It lingers yet in by-places and border districts of Europe, whilst in its old seats in the Orient it still plays havoc with mankind. In India there are two hundred and fifty thousand lepers to-day.

This glance backwards shows how prone are the human tissues to

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the inception of the disease under favorable conditions without distinction of race. But it is in the history of its introduction and course among virgin nations in our own day that this subject may be best studied. The extraordinary rapidity of its spread in the Hawaiian Islands can no more be accounted for on other grounds than its infectious nature, than can the appalling ravages of measles among the people of the Faroe Islands when first imported thither. The peculiar social customs of the Sandwich Islanders will readily account for the vast percentage of those affected. A study of its recent introduction into British Guiana and Trinidad demonstrates in the same conclusive manner that its rapid increase among those peoples could not possibly be accounted for except by infection from person to person. And in still smaller communities this method of dissemination is even more strikingly illustrated, as witnessed in New South Wales, in Panceut, Spain, in Cape Breton, and in its revival in Louisiana. References to such modern instances might be largely multiplied, but these are enough, for if it can be demonstrated that the course of the disease in one nation or in one small community can be reasonably explained only through its contagious or infective nature, we need seek no other explanation of its ravages in all lands and all ages.

2. The study of its action upon individuals.

Extraordinary difficulties stand in the way of the settlement of our question by a positive demonstration of its capability of transference from person to person on account of its long period of incubation. We expose a person to the emanations of variola, and we see the results in a few days ; to contact with another infected by syphilis, and the results declare themselves in a few weeks ; to contact with the materies of tuberculosis, and the effects may be apparent within a few months ; but with leprosy we have reliable data to prove that the disease may not declare itself for years, two, five, or even ten, after a person has been exposed to its influence. Such a retardation of the tangible effects of contagion renders direct historical proof in individual cases almost an impossibility. Again the tissues of other animals than man, already essayed, show themselves utterly indifferent to inoculation experiments, so that we get no response to such methods of investigation. Let us hope that ere long some animal as susceptible to the bacillus of lepra, as is the Guinea pig to that of tuberculosis, may be discovered. The possibilities of using human tissues for such a demonstration in satisfactory sufficiency is too remote for serious consideration. It is true that such experiments have been made. I have seen it claimed that thirty-six persons in Norway and Italy were formerly inoculated and without result, but I am unac-

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quainted with the details of the experiments, whether active bacilli were employed or not, and do not know how long these persons were kept under observation subsequently. The results were simply negative, and therefore without positive value. Who knows that the artificial inoculation of syphilis conducted in the same way might not result in similar failure. Consider the history of such experiments with favus. Before a certain date attempts to transfer the disease from one host to another at will were almost always unsuccessful. Now that we know how to do it, this is accomplished with certainty. One inoculation practiced by a competent person and in a proper manner of recent date has, as you know, been followed by leprosy. You are all familiar with this experiment of Arning at Hawaii, how pus from a leprous ulcer containing bacilli was injected into the skin, and a bit of leprous tubercle was transplanted into the arm of a condemned criminal, who was then isolated, and how three years subsequently leprosy declared itself, and progressed to full development. Unfortunately the experiment cannot be considered absolutely conclusive, as the victim might have been infected beforehand. But it is not likely to be soon repeated.

We must rely, therefore, upon data of clinical nature for our evidence. We shall find this partly in a careful study of the beginnings of endemics of the disease in small communities, as in that of Louisiana, Cape Breton, and Tracadie, as detailed by myself elsewhere, in the reports of Hawaiian physicians of its dissemination throughout those islands, in the account of its importation into a small village in Spain by Zuriaga, and in the cases recently cited by Dr. Loraud at a meeting of the Vienna Dermatological Society. Another class of evidence is that of the acquisition of the disease by immigrants of non-leprous nationality into leprous countries, but of this there are so many instances on record that I need only allude to it. More important is the fact of the frequent development of the disease among the personal attendants of non-leprous stock upon lepers. I will mention a few such instances of recent occurrence. A nun, and a ward-tender in the Guiana lazarettos became lepers. In Russia three clergymen from healthy districts became infected after removing to leprous districts and associating with lepers. In Bergen two ward-tenders have become lepers. In New Orleans a priest has acquired the disease. In Hawaii twenty-five white residents are lepers, of whom two are physicians, and three priests, and Father Damien at last. Twenty of the ward attendants at Molokai have become affected. Who would for a moment think of explaining such occurrences in hospitals for syphilis except through contagion? The case

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of Father Damien alone, to cite the one best known example, is enough to establish the contagious nature of the affection, and to outweigh limitless data of a negative character.

3. Resemblance to other affections generally regarded as contagious.

There are two diseases to which leprosy bears a strong likeness, viz: tuberculosis and syphilis. All three of them are still mighty scourges of the human race, and prevail over all parts of the earth. Two of them have had periods of intensified activity within historic epochs in Europe, raging as vast national epidemics. The other has done its fatal work uniformly and incessantly, never exhibiting such extraordinary and spasmodic effects as to deserve the title of pestilence. They are slow in their development and course, and their effects upon the cutaneous tissues are manifested by the most pronounced lesions the skin is capable of exhibiting. They affect nearly all the other structures of the body as well, showing respectively predilections for special tissues, and eventually profoundly impress the whole economy. The anatomical changes in the cutaneous tissues produced by them, although not identical, are sufficiently analogous to have been placed in one group by pathologists before the common causative element of the same had been demonstrated. One of them has always been regarded as contagious. Another was uniformly considered as such whilst it was most active in its destruction among civilized nations, and until overcome by the universally enforced practice of segregation or isolation mankind outgrew its dread of it, and physicians in later times their knowledge of it. The other has only within the latest time come to be generally recognized to be infectious. All three have been held to be hereditary affections, but as their real nature has become at last understood, it may be said that heredity, although not improbable, is no longer essential to explain their continuance among mankind, for if a disease be proved to be contagious its alleged hereditary character is certainly an unnecessary explanation of its endemic or epidemic occurrence, and contagion is a much more direct and easily understood method of transference from individual to individual than hereditary transmission. But if a disease be both hereditary and contagious, it is far more important to recognize the latter principle, because its effects are more preventable by State and professional control. All three affections are alike essentially of non-venereal nature, although one of them is so frequently transferred through venereal relations. These strong resemblances to each other suggest as intimate relationship in etiology and pathology.

4. Establishment of a satisfactory explanation of its contagiousness.

With the discovery by Hansen of the *bacillus leprae*, the cause of its contagious nature was demonstrated as clearly as was that of

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tuberculosis earlier by the similar discovery of Koch. That of syphilis remains to be as conclusively established. That this has not been accomplished in no measure invalidates the presumption that it will be. We simply await the requisite knowledge of reagents. This will surely come, and then we shall have completed the proof of the close analogy of these three great diseases in all points.

Leprosy is, preëminently, a bacillus disease, for this organism is found in rich abundance in human tissues—chorium, mucous membranes, nerves, blood vessels, ovary, testicle, glands, and the great organs; in the blood, lymph, tears, saliva, nasal and intestinal mucus, milk, vaginal secretions, the discharges of ulcers, and all cutaneous lesions, excepting those secondary to nerve changes. Could any infectious material be more generally or favorably distributed for direct transference to other hosts? How can anyone who admits the infectious properties of the tubercle bacillus reasonably deny the same quality to that of leprosy, or seek a more rational explanation of its extension among mankind? And although we have yet to learn how to make pure cultures of *bacillus leprae*, as we can with that of tubercle, as well as to discover an animal as responsive to its action as is the Guinea pig to that of the latter, yet our ignorance upon these points with regard to the infective materies of that still more contagious third affection, syphilis, is still more profound as yet. Should we ask in these days more substantial proof of the infectious, or contagious, or inoculable character of any affection than the demonstration of its bacillous nature?

5. Influence of control upon its history and course.

Let us now consider what has been the effect of the opposing opinions held from time to time by our profession concerning its contagious character upon its course among civilized nations. You know as well as I the views with regard to its unclean and infectious nature among biblical nations and in mediæval Europe. How the leper was segregated, regarded as a moral monster, forbidden to pass through public ways without a special garb, or bell, or clapper, and how the monks in charge of lazarettos were obliged to wear upon their breast the red letter L. In later centuries when the disease had been thus controlled and confined to a few European districts, the belief grew up, chiefly through the conclusions of Boeck and Danielsson in Norway, and the reports of physicians from various regions, mainly of negative character, collected by Virchow and the English College of Physicians, that it was not contagious, but was endemic because hereditary. Scarcely anyone thought of questioning this dictum. At the meeting of the first International Medical Congress,

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held at Philadelphia in 1876, I called attention to the prevalence of the disease in the United States to a greater extent than was generally known, and again in 1882 I presented a paper upon the subject to the American Dermatological Association. My conclusions that the disease was increasing to an alarming extent in this country, and that it was contagious found then but few supporters among my colleagues. Since then professional opinion has greatly changed. You have heard just now from Dr. Hyde how many important foci of the disease lie within and close around our borders. You know the story of Hawaii and Molokai, the conversion of the profession there largely to the belief in its contagious character, and the fatally tardy efforts of the government to control it by isolation.

In Norway and Sweden, that nursery of the modern doctrine of non-contagion, where the disease had increased in consequence to an alarming extent, a change of policy has taken place. In 1885 the government passed laws authorizing the transfer and confinement within lazarettos of all lepers who are unwilling or unable to be isolated elsewhere in such manner as to prevent contagion in the family. It permits also the compulsory separation of married people. Since then the disease has been constantly diminishing. In 1870 the number of cases in Norway was 2607 ; in 1893 it had been reduced to 800.

In the Russian eastern maritime provinces the disease has become so prevalent that lazarettos have been established in several places. In New South Wales it has increased so much of late among European residents that a law for compulsory isolation was adopted in 1890. In Trinidad there were 3 cases in 1805, 860 in 1878. In British Guiana there were 105 cases in 1860, last year there were 1000 cases.

Thus the disease shows itself increasing rapidly wherever it is granted free license. The late Dr. Morell Mackensie in his essay on "The Dreadful Revival of Leprosy," said : "It is impossible to estimate the total number of lepers now dying by inches throughout the world, but it is certain that they must be counted by millions." This vast destruction of human life and the incalculable misery the disease is producing among mankind is, I believe, a direct consequence of the license which has been granted it by our profession and the nations under this mistaken faith in its non-contagious character.

But how is such unbelief explicable, how is any diversity of opinion as to the communicability of the affection possible in the face of the facts I have thus briefly laid before you ? Why should leper commissions be still seeking some other explanation, and one of the most distinguished surgeons of the day still maintains that it is caused by

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one article of diet, and others cling to the doctrine of heredity as the all-sufficient cause of its continued existence? Partly, in my opinion, because of the prolonged incubation stage of the disease, and the wholly unwarranted reliance upon data of negative character. Just as strong evidence of the kind can be adduced against the contagious nature of tuberculosis and syphilis. X, a leper, lies in a hospital bed next to V with syphilis. Their skins are abundantly occupied by the lesions of their respective affections in all stages of development. Ward tenders, nurses, and physicians care for them with like indifference for months, and ordinarily with no ill results to themselves. The same patients have also lived for months, it may be, in the same condition with their families, and yet the latter have not contracted the disease. Do you not all know this to be true of syphilis in thousands of instances, and yet do you use them as evidence of its non-contagiousness? Why then apply such data to leprosy? On such negative evidence both affections should be equally pronounced non-infectious.

And what sounder evidence have we that leprosy is hereditary? No case of congenital leprosy has been observed, and the disease rarely affects children before the age of five. Leprous parents transmit no cachexia or constitutional degeneration to their offspring. We have no proof that a child born of leprous parents has become leprous, if removed immediately after birth to surroundings free from infection—not one of the descendants of the 160 leprous immigrants to the northwestern states had become a leper at the time of Hansen's visit in 1887, and in but two of the children born at Molokai has the disease appeared. Without denying the possibility of the transmission of leprosy by descent, its apparent occurrence in any instance should not be accepted without the absolute demonstration of the impossibility of inoculation after birth. I believe the theory of heredity to be a very dangerous one, and that the sterner judgment of the Middle Ages, which made the leper individually the responsible agent rather than the progenitor, must again be adopted before the affection can be exterminated. We are dealing every day with infectious diseases, of the infecting properties of which we have no definite knowledge, and yet we should consider ourselves guilty of criminal conduct if we should on this account refuse to acknowledge their contagious character. But in the case of leprosy we do know why it should be contagious, and the conditions which should be favorable to its inoculation. The burden of proof that it can be communicated in any other way is wholly with those of a contrary opinion.

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The Prevention and Control of Leprosy in the United States.

If then we admit the contagious character of the disease, what measures should be taken for its exclusion from, and repression within our country? Is isolation effective, or necessary, or justifiable? Should immigration from infected nations be prohibited? I have already shown the favorable operation of such stringent measures in past times, the fatal error of their negligence, and the recognition of the necessity of their revival by other nations of the present day. What is our own duty in the matter? We have a considerable and ever-increasing number of lepers amongst us, distributed in numerous foci which are known. We have individual cases also scattered over the whole country, a small proportion only of which, I believe, are known to physicians, and this is by far the most dangerous class, because they cannot be shunned. We have all met a few of them. They are free to come and go, and dwell where they like. We shall never control or eradicate the disease without resorting to strong measures, and the sooner they are enforced the better. If we wait until their necessity is more apparent, these results will be of far more difficult accomplishment.

I recommend to the serious consideration of this congress the following propositions:

1. Every physician should be compelled by State law to report to the nearest board of health the existence of a case of the disease, and the neglect to do so on the part of medical attendant or a member of the leper's family should be a penal offense.

2. Immigrants affected by the disease should be arrested at ports of entry and along our borders, and turned back to their previous homes by the authority of the National board of health.

3. Graded hospitals should be established by the National Government, in insular localities where possible, within which both suspected and confirmed cases should be confined, and to which all access should be prevented excepting under proper restrictions.

Such compulsory isolation may be considered cruel to the few, but its longer neglect on our part is certainly a greater cruelty to the many, for in no other way shall we exterminate this most miserable disease. There can be no doubt that the advice of this body would have great influence in determining the action of State and National boards of health in the establishment of such measures for the public safety.

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Dr. George Henry Fox, of New York.

Whatever may be the result of inoculation I am sure that the majority of those who have paid special attention to the study of leprosy, will agree that the disease is contagious in the sense of being communicable, but at the same time a review of all the facts of the case will, I think, convince every one that the danger of infection to those associated with the leper, is extremely slight. In China, in India and among the aboriginal peoples of the Sandwich Islands and other localities, leprosy may spread, but in the United States it has never done so to any considerable extent and this in spite of the fact that for generations past there have constantly been sporadic cases and also communities of lepers in various remote parts of the country. Lepers emigrating from Norway have colonized in Minnesota where many have died and some recovered, but they have not propagated the disease. I am assured by those who have had oversight of this community that in their opinion there has been no harm to the public health.

In the city of New York there have constantly been for generations past cases of leprosy, both in the hospitals and outside them. These patients have mingled freely with the community in all parts of the city and yet there has not been reported the first instance of direct infection. To be sure the public have been periodically panic stricken by sensational accounts of leprosy written for the daily papers and also by ill-advised editorials, one of which I saw in a morning paper in Washington, in which leprosy was unjustly classed with cholera and yellow fever, and all this time the medical profession instead of attempting to allay this unjust prejudice and groundless fear of the disease has done more harm than good by fomenting and augmenting the feeling by a demand, injudicious as it seems to me, for the compulsory segregation of all lepers.

The establishment of a National Board of Health and governmental control of such diseases as are liable to assume an epidemic form is earnestly desired by most of the profession, but I think that there is no necessity for the segregation of the few lepers to be found in the United States. An individual has rights even though he happens to be a leper and these rights should be regarded until it is clearly proven that the individual is dangerous to the general health.

I might cite cases of leprosy affecting men occupying high positions both social and professional, but will not do so.

I will only say that these gentlemen have not infected any members of their families nor any member of the community in which they dwell, nor do I think that they are at all likely to do so; and to take

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these cases and forcibly put them in some government prison would, it seems to me, be an injustice to the individual, which the slight menace to the public health would not warrant.

A case of syphilis is perhaps a hundred times more likely to spread the disease through a community than a case of leprosy. When we take into account the recently developed facts of bacteriology and emancipate our minds from that unjust prejudice which has grown up in the past centuries, I am sure that we shall agree that a leper is no more a menace to public health than is the unfortunate victim of pulmonary tuberculosis. If we were to reverse our ideas of these diseases, I have no doubt that we should go on nurturing our leprous kin and be ready to hound down every case of pulmonary tuberculosis that could be unearthed in the alleys of our cities. As it is now, with strange inconsistency we are going on nourishing thousands of consumptives in the very bosoms of our families and yet raising this terrible hue and cry against a few stray, friendless lepers.

In European hospitals, cases of leprosy are treated in the same wards with other cases of skin disease and I see no reason why that plan should not be carried out in this country. We should of course be glad to have special hospitals established for the treatment and cure of leprosy. I believe that many cases are amenable to treatment and capable of being cured, but the establishment of special hospitals for lepers is different from the establishment of government prisons for their incarceration. If I had a friend who was a leper, I should be sorry to have him put in a government lazaretto such as I have seen on both sides of the Atlantic.

There are dangers far more threatening than those alleged to pertain to leprosy. I admit that it would be far easier to stamp out leprosy than to stamp out syphilis or tuberculosis, but is it not unjust to demand legislative protection against lepers because they are few, while we go on allowing syphilitics and consumptives to increase and multiply in every part of the country and to spread diseases which are quite as loathesome and even as fatal as leprosy, simply because they are so many! To quote the old adage, it seems to me that we strain out a gnat and drink down a camel.

Dr. Joseph D. Bryant, of New York.

The line of consideration allotted to me is not as that to one versed in the medical treatment of leprosy, nor to one who has had unusual opportunity to study its special phases by reason of personal contact with it. These lines of consideration have been given already the deepest of thought, both here and abroad.

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My approach to the subject is as that of one who, by reason of past official experience, has witnessed the immoderate manifestations of fear and dread of many of the citizens of New York when in the recognized presence of but two leprous Chinamen. *Then* the question was asked, what shall be done *with* them? *not* what shall be done *for* them. That which should be done to-day with the lepers of this country is a more pertinent question than at the time of any previous outcry. It is made thus pertinent by past experience and increased knowledge. Still, those who were most clamorous then, maintain a painful silence now, due to the fact, no doubt, that the present health of his Satanic Majesty does not require that he now emulate the virtues of the traditional monk.

It may not be amiss at the outset of this paper, to present a few skeleton facts regarding some of the instructive features of this strange disease. A course of this kind should surely interest the hearers, and it may also serve to emphasize the reiteration of wise sanitary policies long since enunciated.

Leprosy is a chronic, infectious constitutional disease, having a fatal termination. Leprosy had prevailed in certain parts of the world since the time of the earliest history. The accounts in Exodus establish its prevalence among the Jews and Egyptians of that period.

Leprosy was recognized in Greece before the christian era, and it increased there markedly during the last years of that era.

Leprosy, in the early centuries of the present era, extended quite rapidly over the greater part of Europe.

In the seventh and eighth centuries, leper-houses were established in France, Germany and Italy, and edicts against the marriage of lepers were promulgated.

Leprosy reached its height during the eleventh, twelfth and thirteenth centuries, at which time 19,000 lazarettos are said to have been established in Europe. The intercourse incident to the crusades greatly hastened and increased the spread.

During the fifteenth century, it began to diminish, and in the course of the succeeding two hundred years it disappeared almost entirely from the civilized states.

Leprosy exists to-day in Norway, Sweden, Spain, Portugal, France, Greece, Italy, Southern Russia, China, India, Arabia, Persia; also on the islands of the Indian and Pacific oceans. It is present, too, in Japan, Madeira, New Zealand, the West Indies, and to a great degree in the Central, South American and Mexican States, and the Hawaiian Islands.

It is not definitely known when leprosy made its appearance first in

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the United States, although it must have been some time prior to 1785, as at that time a leper hospital had been constructed in New Orleans. At all events, the disease disappeared in a few years and was finally almost forgotten, when it appeared again in the Southern States. It visited South Carolina in 1846, and since this time it has appeared in Louisiana, Minnesota, California, and other Northwestern States. So early as 1815, and perhaps before, it prevailed in the poor French settlements of New Brunswick situated on the Miramachi River. About 130 patients have been treated there in a hospital and lazaretto established for the purpose. At the present time but a few lepers remain. The number of recognized lepers now present in the United States probably exceeds 200. This number, although small when compared with many other forms of communicable disease, nevertheless represents a decided increase.

This disease exhibits no respect for the age, sex, occupation or social condition of those remaining within the circle of its insidious influences. In view of the aforegoing established facts regarding leprosy, that it should be necessary now, in this age of great scientific attainment and general intelligence, to devote even the modicum of time here allotted to the purpose of impressing on the lay minds of this country the wisdom of governmental control of leprosy, is, indeed, a reflection on the foresight and thoughtfulness of our public servants. Much of the inattention and inaction exhibited on their part has been due, no doubt, to the fact, that the medical authorities on this disease have been, and even now are engaged in vigorous contentions as to whether it be contagious or non-contagious in its nature. At all events, as to whether or not it be communicable from person to person as a sequel of the various forms of human intercourse. Although this vexed question, in the opinion of many eminent men, is not yet satisfactorily settled, still sufficient is already known of the disease and its sufferers, to justify, yea, even demand, that prompt and effective measures be taken to resist its further encroachments,—tardy and uncertain though they be,—upon the physical and business vigor of the country.

As yet, a victim of leprosy excites here either a feeling of pity, or dread, or loathing, or of curiosity, perhaps, on the part of the observer; the kind of sentiment evinced being the fruit of the interests, or the environments, or the instincts of the beholder. When, however, for any reason not yet anticipated, this dread disease of sacred history shall smite this country as it did Europe in the eleventh century, then, indeed, will abject fear take the places of pity and dread, and personal and business demoralization those of loathing and curi-

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osity. The proper appreciation of the importance of an early effort to avoid a danger is ever productive of beneficial results, and the history of the day teaches, that those things that but disturb or only threaten the current of business prosperity, or cast a shadow over happy homes, arouse the energy, multiply the vigilance, and stimulate the efforts many-fold of those who by reason of their attainments, can justly be regarded the care-takers of their kind.

The only practical feature of the subject for which I shall claim your attention, relates to the importance of concentrated action directed to the segregation of those in all parts of the country who suffer from leprosy. That which I shall say is not new in any sense, but rather a repetition of facts frequently stated before and quite as frequently disregarded by those whose enduring attention they were intended to excite. That leprosy is communicable by some means from those already afflicted, to those much more fortunate, cannot safely be denied. That the influences of the disease on the healthy, are more marked in the warmer than in the colder climates, is reasonably certain. That the susceptibility to its causative influences is more potent in one section than in another, is past denial. That the restraining influences of this disease are of uncertain tenure, is shown by the devastation it caused in Europe during three centuries of the past era. The reasons for the necessity of active measures directed toward governmental control of leprosy are not a few, and are very forcible.

Firstly—Such a course as this is a humane one.

Secondly—Such a course will beget public security and confidence.

Thirdly—Such a course will secure good sanitation and therapy.

The *humane feature* of this subject demands urgent attention. According to the present status of public sentiment in the great majority of the communities of this country, a leper is an outcast in every sense of the word. None but those of his own nationality will offer him succor, and with these he is not permitted to remain in peace. If a stranger grant him food, he then commands his instant departure, and, wherever he may be driven, there, too, he meets with scant, unchristian notice. For example, it is no doubt within the easy recollection of most of us here that an unfortunate leper who had been taken in charge by the Health authorities of New York, for public policy reasons rather than sanitary ones, to meet the demands of public clamor rather than those of public safety, desired to return to his home in China. His friends were able and willing to bear the expense of the journey. While *en route* to the point of embarkation, his presence was noticed, his disease

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recognized, and his person subjected to the trials and terrors of threatened physical violence, hunger and thirst. Such would be the lot to-day, of a well-meaning and dependent leper, were his presence recognized in a public thoroughfare. To his articulate pleadings there would be given less heed than to the silent expressions of a dumb beast. His natural demands would receive less recognition than would those of soulless brutes. Human sentiment would invoke the law for the prompt protection of the latter, but the human selfishness, bred of fear and base prejudice, would render inoperative the laws of man and God alike, for the protection of the former. The possibility of the exhibition of such manifestations as these, is as sadly inconsistent with the utterances dedicated to American liberty as it is with the teachings of Holy writ. Proper public surveillance of recognized lepers would avert the stigma incident to private and public persecution of them. It would afford both them and the people practical comfort and safety.

The *sense* of *public security* and *confidence* in a community, ought always to be encouraged by diligent cultivation. While it is true that the presence of a leper in New York excites much greater commotion there, than in New Orleans, for obvious reasons, still it is equally true that the presence of a leper in any community should excite a degree of apprehension befitting a proper sense of preservation of persons and interests. There is not a scintilla of evidence to warrant the belief that leprosy has originated *de novo* in New York, nor that other than foreign lepers have yet appeared there. But, as no one can foresee the time when this immunity may be undermined by the subtle influences that permit or provoke the spread of leprosy, the exclusion of foreign lepers from this or any similar community is surely required as an earnest of wise sanitary forethought and decent public policy. It was not until the idea that hereditary transmission alone did not constitute the only means of propagation, that the presence of a leper in New York caused other than comments of curiosity or pity. Twenty-five years ago, the fact of being a leper was not considered of sufficient moment to exclude him from the ranks of nurses in public institutions of that city. To-day, a leper, free within its precincts, is an object of distrust and fear to those who may be brought in contact with him, and is regarded an enemy to public welfare by cautious and thoughtful citizens. The reasons for this change are just and worthy, and in full harmony with advanced knowledge of the danger incurred and a keener appreciation of the value and rights of American citizenship. The race commonly afflicted with the disease in this country are foreign in person and likewise in the social

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and governmental instincts that animate a true American citizen. Although cleanly in person, they are vicious in habits; while diligent, shrewd, and thrifty in their own pursuits, they are unproductive of public benefit, and their inherent phagocytic qualities cause them to entangle and assimilate whatever of use to them, may come within their control. A kind of human maelstrom, that engulfs and swallows everything of use that enters the circle of its influence, and yields nothing here therefor, that is necessary to American comfort and prosperity. Their cause, such as it may be, cannot be nourished and supported by the local public sentiment that unconsciously sustains and shares the afflictions and burdens of a people possessed of homogeneous instincts. Indeed, it is well that such is the case, for if otherwise than this, an infecting colony of foreign unsympathetic lepers would be the reward of public charity, in many of the principal cities of this country.

That good *sanitation* and *better therapy* would result from the segregation of lepers, requires no argument here, as these facts are self-evident to any one who appreciates the sanitary and medical advantages incident to contiguous association and concentrated observation.

Finally the following conclusions are respectfully submitted:

(1) That wise public policy and human instincts demand that lepers be not permitted to associate unrestrictedly with the unafflicted in any community.

(2) That the exercise of local authority for their segregation is unwise and unsanitary, as it establishes innumerable abiding places for them at great expense, and without affording commensurate surveillance and safety.

(3) That a safe, prudent and human policy demands that *irresponsible* lepers be properly segregated under care and control of the general government, and that all others be so restricted in their associations with the healthy as not to invite spread of the disease.

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NATIONAL CONTROL OF LEPROSY.

BY WALTER WYMAN, M.D.,

Supervising Surgeon-General U. S. Marine-Hospital Service.

In the few minutes allotted to me I propose to discuss the relations of the National Government to the subject of the control of leprosy.

In October, 1889, the American Public Health Association, at its meeting in Brooklyn, passed a resolution calling upon the Marine-Hospital Service and the State Board of Health of Florida, and all quarantine-commissioners of ports having intercourse with Cuban ports, to exercise the same vigilance with regard to leprosy that is already observed in the case of yellow fever during what is known as the quarantine-period. In accordance with this request, a circular was issued by the Surgeon-General of the Marine-Hospital Service, approved by the Secretary of the Treasury and the President, December 23rd, 1889, forbidding entry of any vessel to any port of the United States without a certificate from the proper officer that no case of leprosy was found on board said vessel, or in case one had been found, that it had been removed from the vessel and detained at the quarantine-station; with a further provision permitting the departure of the detained leper on outgoing vessels bound to the foreign country from which the leper last sailed.

The new Quarantine Regulations of the Treasury, promulgated April 26, 1894, provide as follows:

"Vessels arriving at quarantine with leprosy on board shall not be granted pratique until the leper with his or her baggage has been removed from the vessel to the quarantine-station.

"No case of leprosy will be landed.

"If the leper is an alien passenger, and the vessel is from a foreign port, action will be taken as provided by the immigration laws and regulations of the United States.

"If the leper is an alien, and a member of the crew, and the vessel is from a foreign port, said leper shall be detained at the quarantine at the vessel's expense until taken aboard by the same vessel when outward bound."

Furthermore, the immigration-law of the United States, providing for the medical inspection of immigrants, forbids the landing of immigrants afflicted with a loathsome or contagious disease, thus furnishing an additional protection against the importation of cases of leprosy. These laws and regulations have, in a measure at least, been effective. I am informed by the Sanitary Inspector stationed at

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Havana that several persons afflicted with leprosy desiring to immigrate to Florida have been debarred from so doing by reason of these restrictions.

National control of leprosy *within* the United States has been frequently advocated, particularly by State and municipal boards of health, when finding such cases upon their hands and desirous of being relieved of their care. The arguments for such control, of course, are based upon the presumption of the contagiousness of the disease (even though moderately contagious) and also upon the claim that where no segregation of cases, or no supervision of cases not colonized is enforced, the disease gradually increases in prevalence, and that where segregation or colonization has been enforced the disease has been made to disappear.

One reason alleged for a national establishment is the fact that in some States the disease is of so rare occurrence that the erection of a special hospital or place of confinement for lepers is scarcely justified, and it is desirable, therefore, that there should be established an asylum to which any of the States might send these unusual but highly objectionable patients. It is further urged that by reason of the difficulty of properly caring for lepers, and because of the uneasiness and excitement that would prevail in cities or localities should the presence of a leper be announced, the health-authorities themselves are tempted to conceal the cases from the knowledge of the public. Thus, if there is anything in the doctrine of contagiousness whatever, the local authorities themselves may directly aid in the extension of the disease. If a national asylum were provided there would be no motive for concealment. Granted that the danger of contagion is small; granted that, in the language of another, a case of leprosy within a family should be regarded with less concern in its relation to the health of the remaining members than a case of tuberculosis; granted that the disease spreads chiefly among the lower classes, nevertheless in the movement which is now only near the starting-point, but which promises to be a controlling movement, and which will mark the close of the present century and the beginning of the next, so far as medical science is concerned, as distinctly as any other evidence of progress in the healing art (I refer to the settled resolution to exterminate every contagious disease), it would appear to be incumbent on the profession to leave nothing undone to exterminate this, together with other communicable diseases.

Now, with regard to national control, there are two considerations involved. *First:* Does the *right* of national control exist? *Second:* If it does, how may that right be best exercised?

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Concerning the first consideration, I find a difference of opinion among eminent men with whom I have conversed, based upon their different views regarding the Constitution of the United States.

A strict constructionist will inform you that the United States Government can only legislate in accordance with powers expressly delegated by the Constitution, and that the general-welfare clause of the Constitution applies as a qualifying clause to the specified prerogatives—that the latter are only granted when necessary for the general welfare.

The States, however, may make any laws which are not forbidden by the Constitution of the United States. This view would discourage the attempt to establish a national leper-hospital by the General Government, but it is claimed that the end might be met by one State establishing a hospital and being willing to receive into it the lepers consigned from other States, their expenses being paid by the latter, as has been done in a number of instances with regard to jails and penitentiaries.

Those who take a different view of the Constitution assert that it is quite within the province of Congress to appropriate a sufficient sum to establish a national leper-hospital, though the necessity for it must be plainly shown. They call attention to laws already passed to which objections similar to those mentioned above were urged.

An outline of a bill for this purpose might be stated as follows:

An Act appropriaing a sufficient sum to establish a national leper-hospital, and authorizing the national officer in charge thereof to receive, or to proceed and take possession of, a leprosy-patient upon the consent of the proper authority of the State. There should be a corresponding act passed by the Legislature of the State, conferring power upon some official—preferably the Governor, with the advice of the State Board of Health—to *respond* to such a requisition. It should be made the duty of the Government officer in charge of the institution, upon hearing of the presence of a leprosy patient, to request such authorities of the State as have been designated for that purpose by the Legislature, to turn over said patient to said officer, for the purpose of being transferred to said hospital; and the Congressional act should also provide that the officer in charge of the institution should make such regulations for the treatment and confinement of the patient as the nature of the case demands.

It will be observed that an act of this nature would still practically leave the determination of the disposition of the patients within a State to the States itself, and leave to the discretion of the Governor and the State Board of Health whether in a given instance the leper should be segregated. The necessity of this discretion is evident from the fact that special circumstances might so surround a given patient as to make it cruel or unnecessary to remove him. At the same time it would provide for the proper removal and proper care of

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those who are not surrounded by such circumstances. To the objection that some States might fail to thus legislate or avail themselves of the privilege of a national institution, it may be urged that in this event the law of February 15, 1893, which permits the making of regulations to prevent the spread of contagious diseases from one State to another, could be brought into exercise to prevent lepers leaving such a State. It is believed that this State would in time have a physical demonstration of the necessity of taking advantage of the national institution or of making equal provision itself.

This is a brief outline, which it might be necessary to modify.

To determine whether such a bill would pass, it would be necessary to introduce it, but, as preliminary to its passage, its necessity would have to be demonstrated to Congress. For this purpose, and that Congress may be assured that the medical profession and sanitary officers had not acted upon insufficient premise, it is suggested that a leprosy-commission should be appointed, of three or five members, to make report upon the prevalence of leprosy in the United States and the necessity and proper method of its control.

A preliminary bill might be introduced empowering the President to appoint such a commission; and, as the success of the bill would be enhanced if it called for no additional appropriation, there might be included a provision sitting aside a portion of what is known as the "Epidemic Fund," to meet the expenses of this commission.

Whether a national leper-hospital would be the result of this action or not, a commission of this character would cause a sense of relief to the people of the United States, whatever its conclusions, either affirmative or negative, as to such establishment.

As for myself, I believe that leprosy should be under national control.

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NEPHRITIS IN ITS SURGICAL ASPECTS.

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PART I.—INTRODUCTORY.

So many inflammations of the kidney have a surgical side that I must define the limits of my subject. I shall speak only of suppurative pyelonephritis, that common malady sometimes called "the surgical kidney," and I shall consider it both as it occurs spontaneously and as a complication of vesical disorder, the drift of my discussion to be a consideration of etiology and of the practical means of preventing the malady.

The subject, though well worn, is not threadbare. Investigators are still struggling with its problems, although much light has been already thrown into its dark corners—light which is reflected in the routine methods of the practical surgeon of to-day, enabling him to conduct his patient more safely past points of danger than was in any way possible a few years ago.

Much of this light emanates from laboratorial researches carried on abroad—notably in France, under the inspiration of Guyon—and from these researches I must borrow largely in introducing my subject.

It has been contended that the name "surgical kidney" is a misnomer (C. Lucas), since its existence is often due to the lack of intelligent surgery, and that it would be as logical to name a riot a "policeman's mob," because it was due to the absence of that enforcer of the law; but the distinction is captious. The term may well remain, since the surgical kidney is a complication for the most part of surgical disorder, and its study within the domain of surgical pathology.

Modern demonstration shows in a manner which the logical mind must accept that suppuration in the kidney, as elsewhere, is associated with the existence of micro-organisms, and due directly to their presence. Yet a distinction is here necessary, as it is, for instance, in the contemplation of contagious disorders, for the production of which, as Bouchard aptly puts it, something more is necessary than the accidental encounter of a man and a microbe; the soil must be suitable, the conditions must be favorable, or the seed does not take root.

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I cannot discuss the controversial aspect of microbic infection, as this paper aims at being practical, but must ask you to accept, what wide demonstration at many hands has proved, that without the coöperation of microbic infection there is no suppuration in the urinary passages.

Since Pasteur opened the subject in 1859—less than a lifetime ago—by noticing that boiled urine remained sweet, while dust brought decomposition to it, and, with Van Tieghem, fastened the blame upon what he called "torules ammoniacales," all through the line of Traube, Cohn, Fels and Ritter, Hiller, Niemeyer, Klebs, Lister, Lancereaux, Lepine, Roux, Albarran, Hallé, Krögius, Schnitzler, Rovsing, Clado, Chabrié, Escherich, Morell, Achard, Renant, Reblaud, Guyon, and a host of modern observers the demonstration has been progressive, and no escape has been possible from the flat conclusion: In the urinary tract, no microbes, no suppuration. The protest of Enriquez is by the way, together with that of Manotti and Bachiocchi and others—for that even normal urine may contain germs, and that the kidney may eliminate such, without the necessity of accompanying renal inflammation, may be granted; we have only to revert to Bouchard's aphorism of the man and the microbe: The seed may be old, it may be unsuitable; the soil may not be ready; surely every man in an epidemic does not acquire disease.

The healthy urethra is well known to be the habitual residence of numerous micro-organisms, some of them pathogenic, just as the external integument is; yet they cause no disease under normal conditions. So much at home are they indeed, and so intimately bedded among the cells and in the ducts of the mucous glands, that no amount of prolonged irrigation with hot water, boric acid, or even 1 in 1000 nitrate of silver solution at the hands of Petit and Wasserman could succeed in rendering this membrane sterile; inoculation of its mucus (after irrigation) in bouillon tubes always gave positive results.

This, at least not aseptic, condition of the urethral mucous membrane stops in health at the cut-off muscle. Beyond and above all is pure, and Guyon with his pupils, as well as others, have amply demonstrated that urine taken aseptically from the bladder is sterile.

How and why, then, in cases of suppuration does it become contaminated?

The *How* is clearly by way of the introduction of micro-organisms, either by (1) propagation along the mucous surface by reason of local damage to the deep urethral membrane [tubercle, cancer, stone, bruising violence, erotic excitement, gonorrhœa, gouty congestion, enlarged prostate—or spontaneously]; (2) introduction by the surgeon upon his

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instruments; (3) bursting into the urinary tract of some extraneous focus of suppuration; (4) transmission through the unbroken tissues from a neighboring focus—it has been even alleged that the bacterium coli commune may get through from the rectum and do its work upon the mucous membrane of a bladder ready to receive it (5) by descent from the blood downward through the kidney—but this, except the wedge-shaped pyæmic foci, is very uncommon, and, as in Bouchard's suppurative nephritis occurring during the course of a general infectious malady, the suppuration commencing in the kidney, usually remains there and does not descend into the bladder.*

I cannot halt here to discuss the various alleged methods of invasion, but pass on to the more important *Why?*

Why do micro-organisms produce suppuration in some instances and not in others? This opens the question of preparation of the soil for the seed.

As early as 1873 Fels and Ritter, by inoculating the bladders of dogs, produced ammoniacal urine and cystitis—but only on condition of ligating the urethra. Upon loosening this ligature the bladder promptly resumed its condition of health. So Guyon, Albarran, Guiard, and many others, introducing pure cultures of micro-organisms into the healthy bladders of animals, fail to set up cystitis unless to the microbian germ there to be added other factors, such as ligating the urethra to produce forced retention, or wounding the bladder. Certain micro-organisms have proved themselves more virulent than others, just as certain subjects are exceptionally susceptible, and Schnitzler claims that with the "urobacillus liquefaciens septicus" he can produce cystitis without tying the urethra. This urobacillus is now believed to be identical with the bacterium coli commune; more evidence is therefore needed. Barlow has recently contributed some testimony on this point.

But Guyon, Petersen, Albarran, and many others have repeatedly proved that retention, ligating the urethra, traum—not one of these causes alone will produce cystitis, and that anyone of them plus the proper germ will do it. Straus and Germont clearly proved that simple ligature of the ureter, aseptically performed, does not occasion inflammation of the kidney, but produces dilatation, and then atrophy; while Charcot and Gombault, with equal clearness, have demonstrated that septic ligation of the ureter does produce the suppurating kidney.

That trauma prepares a soil for microbic infection is beautifully illustrated by Albarran. He showed that the blood might be a channel of infection by inoculating one ureter and ligating it below; then

* Albarran has produced descending pyelitis experimentally.

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finding both kidneys implicated as being spots "minoris resistentiæ"—the ligated side, because of congestion and of direct infection, the other, on account of its hyperactivity from having had double work to do. Then Albarran injected his bacterium pyogenes directly into a bloodvessel in a number of animals, and got plenty of embolic abscesses in every instance, but found the kidneys free in all save one. To study the effect of injury plus germs, he therefore contused one kidney in a rabbit and injected a pure bouillon culture of bacterium pyogenes into its ear. The next day this kidney was already in commencing suppuration. He produced nephritis also by injecting the ureter (and ligating) with streptoccus pyogenes, and with staphylococcus aureus as well.

Indeed, how many kinds of germs there may be capable of setting up suppurative nephritis, I do not know. That there are a number seems certain; but there still exists confusion among authorities, and the last word has not been spoken. Nor is the method of action quite agreed upon, Rovsing believing that ammoniacal fermentation of urine is a factor of prime importance, while Guyon upholds that this feature is secondary. That it is not always essential is shown by the fact that there is such a thing as acid cystitis. Uncomplicated tubercular cystitis is an example in point.

As for the single germ named by investigators seeming to have the most pernicious qualities, it appears to be growing more and more clear that this germ is the bacterium coli commune,* a bacillus similar to, if not identical with that commonly found in the colon under all circumstances.

This it seems is the same as the urobacillus septicus, and as the septic bacterium discovered by Bouchard in 1879, by Clado, and by Hallé in 1887, studied intimately in 1888 by Albarran and Hallé, and by Albarran in 1889 as the bacterium pyogenes, by Rovsing in 1889 as the cocco-bacillus ureæ pyogenes, by Chabrié in 1892 as the urobacillus non-liquefaciens septicus, by Morell as the bacterium lactis ærogenes, by Krögius, Achard and Renant, Reblaud, etc.; all these seem to be the same time-honored bacterium coli commune. This germ is most often found in bad cases. Associated with it, or alone, may be found also the staphylococcus pyogenes aureus and the streptococcus pyogenes.

Accepting then, from experimental data, that suppurative inflammation along the urinary tract demands for its production, first, a microorganism; second, a soil properly prepared by over-distention, con-

* 4 to 6μ long, 2μ broad, with rounded ends; staining by Weigert's method.

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gestion, or trauma, let us next turn to the clinical side of the picture, and see whether the same conditions obtain.

Surely they do. The young and vigorous bladder and kidney do not suppurate except for trauma—injury, stone, tumor, stricture, gonococcus, tubercle; in weakly young subjects they do suppurate when depressed nerve force and atony of the circulation, or intense congestion from repeated erotic cause, may be presumed to prepare the way through the congestive process. We all know with what liberty young subjects may be handled instrumentally. The slight disturbance following sounding or catheterism quickly subsides; urethral fever and surgical kidney are here almost unknown.

Again, in youth and vigorous manhood, when the bladder is strong and capable of emptying itself, when there is no atony—in cases of retention from fever, coma, spasmodic stricture, with what carelessness the catheter may be used, and no evil results follow; and even in such cases, although there be added traumatism and congestion produced by stone, stricture, gonorrhœa, or even tubercle; here, too, in the young and vigorous, even moderate care and cleanliness yield results in the main satisfactory; high grades of inflammation are not produced by careful instrumentation, and ascending pyelitis is wholly exceptional. But in these cases a muscularly healthy bladder is presumed to exist, one capable of demonstrating, by frequently and entirely emptying itself, the value of Guyon's maxim, "the bladder is the guardian of the ureters."

But how does the picture change when in waning life, be it premature or in the fulness of time, the nerve force fails, the bladder loses its expulsive tone, its muscular wall becomes replaced by tissue more or less fibrotic, crowding out and atrophying the muscular fibre, when there is more or less residual urine, when the ureters are dilated. Now let there be occasion for the use of any instrument in the bladder, and it requires often the utmost skill and attention, with all the asepsis and antisepsis which we can command, to avert septic conditions which readily climb the ureter through the door thrown open by previous preparation of the soil, and, producing the true surgical kidney, terminate the life of the patient, positively by reason of the surgeon's interference. Here the soil is rich and ready for the seed—the surgeon plants the seed in the urine, until then clear, and the patient reaps the harvest.

The physical soil is prepared by anything that weakens the bladder, congests or bruises its surface. Thus all antecedent conditions of disease for which the aid of the surgeon is invoked—stricture, prostatic enlargement, stone, tumor, retention—prepare the soil more or less, and it behooves the surgeon to be watchful about sowing the seed.

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Turning again to the experimental field, let us study briefly the effect of the most active of the causes which prepares the way for kidney invasion from below. This cause is retention of urine or bladder tension, anything that interferes with the free and perfect passage of the urine downward from the kidney.

James has determined the normal urinary pressure within the ureter to be about two and one-half centimeters of mercury, and Hermann has shown that a pressure of six centimeters almost suppresses the urea in the urine. Guyon, Albarran and others, have proved by experiment upon animals, that sudden closure of the ureter does not lead to a distention of this canal. The kidney does become distended, then some of its contents are reabsorbed, next the kidney undergoes atrophy. This circumstance caused Guyon to suggest that when the ureter was accidentally severed in some abdominal operation, the duct should be ligated aseptically and the kidney left to atrophy, thus saving the patient a needless nephrectomy. The kidney will not suppurate. Crooke found this atrophy of the kidney to have taken place in a female where the ureter had been aseptically occluded by a cyst of the broad ligament. The ureter was not dilated.

All this question of the effect upon the kidney of rapid closure of the ureter and of the slower process of obstruction below has been experimentally elucidated by Guyon upon dogs.* Absolute aseptic ligature of the ureter leaves the duct unaltered, causes dilatation, then atrophy of the kidney, the papillæ becoming flattened, the tubes dilating, the kidney substance getting sclerotic, and then withering. Tension in the kidney at first increases quickly, as shown by the mercurial manometre, to 40 millimetres in twenty minutes, and to 73 in an hour, dropping again swiftly to 44 millimetres in four and a half hours. After twenty-six days it becomes 11 millimetres, and after sixty-two days to 3 millimetres, which is about what James has shown to be the normal pressure.

When, however, the obstruction is less acute, as when the urethra is ligated, polyuria is a first result, with a progressive diminution in the quantity of urea contained in the urine, shown by the fact that the urine in the bladder always contains more urea than that taken from the ureters, the latter more than can be obtained from urine found in the pelvis of the kidney, circumstances which go to prove three things: first, that the dilatation of the ureter takes place from above downward; second, that the diminution in the secretion of urea is progressive, and third, that regurgitation of urine does not take place; what was first excreted goes to the bladder, the last remains in the

* Experiments conducted by Allbaran and Chabrié.

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kidney. In vesical hæmaturia from acute retention of urine the fluid in the ureters remains pale, that in the bladder being bloody.

The slowly progressive dilatation of the ureter from above downward, and not from below upward, is admirably brought out in the thesis of Hallé.

In some cases of stricture, when the vesical muscle continues powerful, it is probable that the ureters are forced, because their orifices are found widely dilated, but in most instances, and notably in the case of thin-walled, weak bladders, or in thick-walled, sclerosed, weakened bladders, where there is atony, the ureter, although widely dilated, has a very small vesical orifice. Moderate obstruction below to free urinary outflow, even painful vesical spasm, is believed to be capable of leading to dilatation of the ureters, and of causing the renal changes incident thereto, changes which make the kidney resemble a senile organ, and prepare it fully for microbic invasion.

A difference, however, is here. In the senile organ, partly atrophied and partly sclerosed as it is, there is an abasement of function. A number of observers agree upon this point. I may cite notably the thesis of Pierre Roche, who concludes that in the aged the quantity of urine is reduced one-sixth, the phosphoric acid is diminished by two-thirds, urea by one-half, chlorides unaltered, density about normal.*

Such abased function is not found in the kidney damaged by gradual chronic over-distention. Here there is an irritative element. Polyuria, very active polyuria, worse at night, is the rule. Ultzmann makes polyuria a constant sign of chronic pyelitis; and Oppolzer has even gone so far as to say that many cases of diabetes insipidus are only instances of chronic pyelitis unrecognized.

I personally look upon this sign, polyuria, in an old man, with ever-increasing respect. An individual with this symptom markedly present I consider the worst of all possible risks in case of operation. I greatly prefer to operate upon the bladder of a man having intense suppurative pyelitis on both sides, whose urine is full of casts and loaded with albumin, and I expect him to get well after a serious operation with much more hope than I do a patient having clean, light, thin urine, containing no albumin, or only a trace, and either no casts or only an occasional hyaline one, but who presents this symptom, polyuria, to a marked extent.

* Roche, following Bouchard, puts the daily average secretion of urine in old age at 1350 grammes, Rayer at 1824, c. c., Becquerel at 1267, Vogel between 1400 and 1600, Gubler between 1200 and 1500, Hepp at 1500, Beigel at 1320, Ballet from nineteen observations (150 tests) at between 500 and 800 grammes, his patients being from 70 to 86 years old. Therefore, polyuria is not a natural condition in old age. Its existence means disease. Albumin is not a normal ingredient of the urine of old age.

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Cases therefore suffering from the results of slow prolonged obstruction below, and with dilated ureters, be their condition due to stricture, prostatic disease, stone, or old age, are subjects ripe for suppurative infection of the kidney, brought on either spontaneously by a mounting upward of the microbic infective agent from below in the natural course of progressive suppurative disease unrelieved by treatment, or by an implantation of the seed through the instrumentality of the surgeon.

For although the dilatation of the ureter takes place from above downward, yet after it has occurred, micro-organisms introduced into the bladder, and even particles of inert solid matter so introduced, shortly find their way upward, and may be recovered in the pelvis of the kidney, as has been demonstrated at the hands of several investigators. Inorganic matter, such as coal-dust, mounts slowly; organic matter (micro-organisms) rapidly.

The deductions to be drawn from this brief summary of facts are roughly:

1. To use reasonable care in exploring a healthy bladder or passing any instrument into it.

2. To use greater care if there be traumatism from stone, tumor, stricture, especially if the powers of the individual be weakened by age or disease.

3. To exercise every known precaution in exploring and manipulating, instrumentally, cases of dilated bladder in a fibrotic state, with enlarged ureters and damaged kidneys.

Part II.—Practical Application of this Résumé.

I now reach the practical question: *How* shall the kidney be guarded?—for prophylaxis is more than cure—a kidney once in positive *chronic* suppuration does not get well. Acute suppurative cases very often die; less active ones suppurate indefinitely, and, eventually kill, by the process of urinary cachexia, so-called, in most instances.

The ends to be kept in view are (1) to avoid all unnecessary traumatic violence, and (2) to maintain asepsis, particularly, when the urine is clear, and more especially so when there is a weak bladder, residual urine, and dilated ureters.

When the bladder and kidney are already in chronic suppuration the indication is to attempt to destroy micro-organisms by direct antiseptic applications below, and to sterilize the urine by medicines from above. This means an attempt at asepsis in instrumentation, antisepsis in topical and in internal medical treatment.

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Although these ends may be aimed at, none of them can be absolutely reached clinically;* for let instrumental asepsis be carried even to perfection, yet the pendulous urethra harbors a multitude of germs, some of them pathogenic; and this anterior urethra cannot by any known process be absolutely clarified; therefore, be the instrument ever so clean, it may push some noxious germ before it into the bladder, where, finding proper pabulum, it may multiply to the patient's discomfort; in this way the victim being, as it were, "hoist by his own petard." Fortunately, the surgeon can kill this germ, and often Nature can dispose of anything short of massive infection.

On the other hand, it is absolutely impossible to disinfect a bladder in chronic suppuration by any antiseptic irrigation, since many of the micro-organisms lie concealed, imbedded in the cells, protected from the action of the antiseptic lotion. Finally, medicines acting through the stomach do not effectively destroy micro-organism in the kidney —although they help in the main issue.

But shall we on account of these rebuffs fold our hands placidly and let Nature struggle unaided? By no means. Indeed, we can do very much, in a helping way, as daily experience attests. With modern methods it is nearly possible to do away with urethral and urinary fever. An old prostatic with thin bladder, clear urine, atony, dilated ureter, and damaged kidney, may be so handled as to escape catheter fever entirely, and to emerge into catheter life with sparkling urine and no pyelitis.

How shall this end be attained? The complicated methods of asepsis which have been advocated cannot endure; they must be made simple to become generally adopted. Machines for forcing sulphurous acid fumes or steam through catheters are well enough for specialists and special occasions, but they cannot be broadly applied. Boiling water (or dry heat for solid instruments), bichloride of mercury or nitrate of silver, 1 in 1000, are all that is required. Woven instruments should be discarded as far as possible on account of the difficulty of keeping them clean. All instruments after use should be subjected to boiling water, hollow instruments injected with alcohol

* Nor are all persons equally susceptible, moderate interference producing chill in one, when another, apparently in a more receptive condition, escapes entirely. Thomson, a medical missionary, states, that among the Chinese urethral fever is unknown. Stricture and vesical calculus, he thinks, are more common in the province of Kwang-Tung than anywhere in the world, and in the hospital sounding and catheterism are practised daily, yet, he says, "in not one single case have I observed the fever that so frequently is concomitant of similar operations in Great Britain." He ascribes this to Chinese phlegmatic temperament, and apparently uses no preventive precautions.

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before use and soaked for an hour in bichloride or twenty minutes in nitrate of silver solution.

But it is not essential, except on special occasions, to use all these precautions. For an operation or a first exploration in an aseptic case they are essential. It is all important always to avoid traumatic violence as far as possible. This factor, according to my experience, is more important than very high class asepsis. Moreover, in the young and healthy, simple cleanliness may suffice; in the old and feeble asepsis and antisepsis are essential. Then again in the great majority of those in whose behalf instrumentation is called for, chronic suppuration already exists, and it is hardly logical to strain at attempts to avoid befouling a pool which is already defiled. Therefore simple but perfect cleanliness (hot water) and the avoidance of traumatism are enough in most instances, except when an operation is to be performed, an exploration to be made, or catheter life started upon a patient whose residual urine is clear. Here rigid asepsis and antisepsis are desirable. I have no respect for that minute detail sometimes advocated which counsels a bichloride towel and disinfection of the meatus for every simple passage of the sound, and strives to render an old man's catheter life still more intolerable by forcing him to sterilize a number of catheters, to keep them in glass tubes stopped with cotton, and to disinfect his meatus at every instrumentation. Few old men will or can continue to do this, nor is it necessary.

I see no advantage in sterilizing or borating vaseline or glycerin. I had cultivated some vaseline taken from a pot in use for twenty years. This pot had never been disinfected or sterilized or its contents medicated in any way. It had been wiped out when empty, refilled from a supply can, and kept generally covered. I had some glycerin similarly cultivated. It was taken from a bottle never sterilized or medicated in any way, and in use for many years for catheter, cystoscope, and endoscope. These specimens yielded on cultivation only some ordinary air species of germs—nothing pathogenic.

The method of instituting catheter life which yields me all the satisfaction I require is the following: A rubber catheter is kept constantly in a 1 in 4000 bichloride solution and washed in very hot water after each use. Simple glycerin is the lubricant. The anterior urethra is to be flushed as the catheter enters for the first time and all the way in through the membranous and prostatic urethra with a 1 in 4000 bichloride solution. The urine is to be entirely but *very slowly* drawn off (watching the pulse), the patient being recumbent. The bladder is then to be immediately washed out with a 1 in 1500 (sometimes 1 in 2000, if the stronger causes too much pain) nitrate of silver

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solution, which usually occasions moderate temporary tenesmus. The bladder may then be washed out with 6 per cent. salt solution (heaping tablespoonful to the quart), this to be drawn off, and such quantity of a weaker salt solution (teaspoonful to the quart) introduced and left in as the surgeon may think best.

The clean catheter taken out of its bichloride solution and rinsed in hot water and the salt solution are to be used as often as any catheter is called for—further urethral flushing being a matter of personal judgment—the nitrate of silver repeated every one, two, or three days.

With such simple precautions catheter life may be safely instituted without implicating the kidneys, and often (unless the bar at the vesical neck bleed from being bruised) with (practically) an absence of pus in the urine, and this goes on until the tissues become hardened, when a non-disinfected catheter may be safely used, if simply kept clean.

When an operation is to be performed the urine is usually already purulent. In this instance it is better, and it has become almost a routine matter with me, to flush out the entire urethra and bladder before the operation with a salicylic acid solution followed by a 1 in 3000 or 4000 bichloride solution (this after the anæsthetic has been administered), and to follow the operation by a flushing out with a nitrate of silver solution, 1 in 1000; finally, and in subsequent washings for cleanliness, using about a 6 per cent. solution of common salt in hot water, and such strengths of mercuric or argentic irrigation as seem called for.

I have discarded Thiersch's solution and boric acid for the same reason, difficulty of prompt preparation. If the Thiersch solution be made on the spot (salicylic acid, ʒ ss; boric acid, ʒ iiiss, in powder, added to a quart of hot water, is near enough to the formula), it dissolves slowly. If the cold solution has to be heated it is troublesome. Boric acid also dissolves slowly, and the bacteriological experiments soon to be detailed show that it is no more effective in killing germs than is salt and water. Now salt dissolves at once, and it is more cleansing than simple water, therefore I use it; and salicylic acid besides being an admirable germ killer, as the bacteriological tables will show, has the extra advantage, first pointed out by Bryson, of St. Louis, of penetrating further and cutting deeper into the thick cohesive muco-pus of chronic catarrhal inflammation than any other substance. Therefore I have had made a solution of salicylic acid in alcohol, eight grains to the ounce. The bacteriological tables show that one-half grain salicylic acid to the ounce is as

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effective a germ killer as Thiersch's solution, consequently, by adding one ounce of my alcoholic salicylic acid solution to a pint of hot water, I have a mixture promptly made and as effective as that of Thiersch.

These are the solutions, and this the routine method upon which I rely for avoiding kidney complications when surgical interference upon the bladder is called for.

Medicines to affect the urine and the flushing of the kidney by diluent waters—adjuvants of no mean value in keeping down kidney complications and moderating them when they exist in a chronic form—I shall speak of after I have detailed the result of the bacteriological work, which forms an essential part of this study.

Part III.—The Bacteriological Study.

In order to investigate the bacteriological side of this question of kidney and bladder infection, with consequent suppuration, and to see what confirmation or dissent would be furnished to the conclusions which clinical observation justified, I determined to institute a rather extensive series of experiments, and obtained the coöperation of Dr. E. K. Dunham, Professor of Histology, Bacteriology, and Hygiene in Bellevue Hospital Medical College, a gentleman whose position insured his disinterestedness and capacity. I selected nearly all the substances I was accustomed to use either for local application or through the stomach, and asked him to determine their relative destructive power, if any, upon pure cultures of staphylococcus pyogenes aureus, of streptococcus pyogenes, and of bacterium coli commune. I incorporate his tables and his remarks as to the process employed.

The substances chosen to be experimented with * are the following :

1. 1 in 1000 mercuric bichloride.
2. 1 in 2000 " "
3. 1 in 3000 " "
4. 1 in 4000 " "
5. 1 in 5000 " "
6. 1 in 10,000 " "
7. 1 in 500 argentic nitrate.
8. 1 in 1000 " "
9. 1 in 2000 " "
10. 4 per cent. solution of boric acid.
11. 10 per cent. solution of common salt.
12. 20 " " " "
13. 30 " " " "
14. Thiersch's solution.
15. Acid salicylic, gr. ss; alcohol, ʒ ss; water, ℥ j.
16. Marchand's peroxide of hydrogen, 1; water, 3.

* I did not test permanganate of potash or carbolic acid, as I very rarely use them.

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All these were to be tested for various lengths of time. Vaseline also was furnished from a pot not prepared, not disinfected in any way, and in common use for many years, and common glycerin from a bottle which had been in use for many years.

I supplied also many medicated urines. I selected two individuals in apparent good health whose conduct I could control. They took the same medicine at the same time, and at intervals of about four days specimens marked A. B.: A1, B1, etc., were sent to the laboratory for investigation, Dr. Dunham being at the time ignorant of the kind of medicine that was being experimented with.

I was guided as to my interval of time between the tests by observing that the greenish-blue color imparted by methylene-blue to the urine, being most dense about eight hours after the drug was swallowed, practically disappeared during the fourth day, although faintly visible upon the fifth—an overlapping that could not be important. About five thousand examinations in all were made.

REPORT ON A SERIES OF EXPERIMENTS TO DETERMINE THE VALUE OF CERTAIN SOLUTIONS AS GERMICIDES, MADE FOR DR. E. L. KEYES IN OCTOBER AND NOVEMBER, 1893, BY EDWARD K. DUNHAM.

The solutions experimented with were, first, solutions of chemical substances made of definite strength, distilled water being used as a solvent; second, samples of urine from healthy individuals after the exhibition of certain drugs.

The bacteria used to test the germicidal value of these solutions were: Staphylococcus pyogenes, aureus, streptococcus pyogenes, and bacterium coli commune. The cultures used were either vigorous cultures in bouillon grown in the incubator at a temperature of about 36° C. about twenty hours, or a similar culture upon agar-agar, cultivated at the same temperature for the same length of time.

The experiments made can be divided into two groups, which were designed in one group to favor the action of the substances in solution by bringing them into the most intimate contact with the bacteria experimented on; while in the other group of experiments an attempt was made to favor the bacteria by necessitating the penetration of the solution through a thin layer of bacterial growth before contact with all the bacteria could be effected. The details of the procedure were as follows:

GROUP I. Five cubic centimetres of the solution, or urine, were introduced into a sterile test-tube by means of a sterilized pipette. One drop of the twenty-hour bouillon culture of the bacterium was then introduced and the mixture well shaken. One loop

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(circa one drop) of the mixture was then removed after a definite interval of time, and either introduced into a tube of melted gelatin which, after mixing its contents, was placed in a horizontal position until the gelatin had solidified, or the loop of the mixture of solution and bouillon culture was distributed over the surface of agar-agar solidified obliquely in a test-tube. The growth which took place in the gelatin or agar tubes was then observed and recorded.

GROUP II. A series of test-tubes in which agar had been solidified obliquely were inoculated with the bacteria to be experimented upon, and placed in the incubator for about twenty hours. Into these tubes enough of the solution or urine to be tested to completely cover the growth of bacteria was poured. After a given length of time the solution or urine was poured off, the layer of growth washed with about ten cubic centimetres of sterile bouillon, and then either a gelatin tube or an agar tube inoculated with some of the growth remaining in the tube. On successive days the original agar tubes, the bouillon with which they were washed out, and the gelatin or agar tubes inoculated from them, after the washing with bouillon were all examined for evidence of growth and amount of growth, and the results recorded.

The urine received for use in these experiments was not free from living bacteria. It, therefore, was necessary to make sure in each case that the growth of the various culture tests were of the species intentionally experimented upon. In the case of the staphylococcus pyogenes aureus a simple inspection of the growth upon agar was deemed sufficient to decide this point. In the case of the streptococcus the growth was examined in hanging drops, and if the coccus chains containing the individual cocci of the proper dimensions were observed a positive result was recorded. As a matter of fact, the growth when present was in nearly all cases sufficiently typical to leave no room for doubt as to the identity of the species. To test for the presence of living bacterium coli commune, gelatin cultures were chosen, and if white non-liquefying colonies of proper size, accompanied by gas production developed, the result was recorded as positive.

Some of the mixtures were allowed to stand for the specified time at the temperature of the room, which varied from 65° F. (16° C.) to 72° F. (22° C.)

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Experiments with Chemical Solutions and Medicated Urine.

o no growth.
x slight growth.
xx moderate growth.
xxx considerable growth.
xxxx vigorous growth.
† Test defective.

Solution used.	Bacterium	Group I. Time of exposure, and result.				Group II. Time of exposure and result.			
		2 min.	10 min	1 hr.	24 hrs.	2 min.	10 min	1 hr.	24 hrs.
Mercuric chloride, 1 in 1000	Sta. p. a.	o	o	o	o	x	x	o	o
	Strep.	o	o	o	o	o	o	o	o
	Coli.	o	o	o	o	o	o	o	o
1 in 2000	Sta. p. a.	x	x	o	o	x	x	x	o
	Strep.	x	o	o	o	o	o	o	o
	Coli.	o	o	o	o	xxxx	xxxx	x	†
1 in 3000	Sta. p. a.	xx	x	†	o	x	x	x	o
	Strep.	o	o	o	o	o	o	o	o
	Coli.	x	x	o	o	†	xxxx	xxxx	†
1 in 4000	Sta. p. a.	o	o	o	o	xxxx	xxxx	xxxx	†
	Strep.	o	o	o	o	†	†	†	o
	Coli.	o	o	o	o	†	†	†	†
1 in 5000	Sta. p. a.	x	x	x	o	xxxx	xxxx	xxxx	†
	Strep.	†	†	o	o	o	o	o	o
	Coli.	x	x	x	o	xxxx	xxxx	xxx	†
1 in 10000	Sta. p. a.	xxxx	xxx	xx	o	xxxx	xxxx	xxxx	xxx
	Strep.	o	o	o	o	xxxx	xx	xx	x
	Coli.	o	o	o	o	xxxx	xxxx	xxx	xx
Argentic nitrate, 1 in 500	Sta. p. a.	o	o	o	o	xxxx	xxxx	xxxx	†
	Strep.	o	o	o	o	†	†	†	o
	Coli.	o	o	o	o	xxxx	xxxx	xxxx	†
1 in 1000	Sta. p. a.	o	o	o	o	xxxx	xxxx	xxxx	†
	Strep.	o	†	o	o	xxxx	xxxx	xxxx	†
	Coli.	o	o	o	o	xxxx	xxxx	xxxx	†
1 in 2000	Sta. p. a.	o	o	o	o	xxxx	xxxx	xxxx	xxx
	Strep.	o	o	o	o	xxx	xxx	xxx	o
	Coli.	o	o	o	o	xxxx	xxx	xxx	xx
		1 min	2 min						
Mercuric chloride, 1 in 4000 at 98° F.	Sta. p. a.	x	x						
	Strep.	o	o						
	Coli.	x	o						
Argentic nitrate, 1 in 1000 at 98° F.	Sta. p. a.	o	o						
	Strep.	o	o						
	Coli.	o	o						
		2 min	10 min						
Boric acid, 4 per cent.	Sta. p. a.	xxxx	xxxx	xxxx	xx	xxxx	xxxx	xxxx	†
	Strep.	xxxx	†	o	o	xxxx	xxxx	xxxx	†
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	†
Sodium chloride, 10 per cent.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	†
	Strep.	xxxx	x	o	o	xxxx	xxxx	xxxx	†
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	†
20 per cent.	Sta. p. a.	xxxx	xxxx	xxxx	†	xxxx	xxxx	xxxx	†
	Strep.	xxxx	xxxx	o	o	xxxx	xxxx	xxxx	†
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	†
30 per cent.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	†
	Strep.	xxxx	xxxx	o	xxxx	xxxx	xxxx	xxxx	†
	Coli,	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	†
Thiersch's solution,	Sta. p. a.	x	o	o	o	xxxx	xxxx	xxxx	†
	Strep.	o	o	o	o	xxxx	xxxx	xxxx	†
	Coli.	o	o	o	o	xxxx	xxxx	xxxx	†

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Solution used.	Bacterium	Group I. Time of exposure, and result.				Group II. Time of exposure, and result.			
		2 min.	10 min	1 hr.	24 hrs.	2 min.	10 min	1 hr.	24 hrs.
Salicylic acid, ½ gr. in 1 oz.	Sta. p. a.	o	o	o	o	xxxx	xxxx	xxxx	†
	Strep.	o	o	o	o	xxxx	xxxx	xxxx	†
	Coli.	o	o	o	o	xxxx	xxxx	xxxx	†
Marchand's peroxide of hydrogen, 25 per cent.	Sta. p. a.	x	o	o	o	xxxx	xxxx	xxxx	†
	Strep.	o	o	o	o	xxxx	xxxx	xxxx	†
	Coli.	x	o	o	o	xxxx	xxxx	xxxx	†
Unmedicated urine, A[1]	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	†	†	†	†	†	†	†	†
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
A.I. 6 grs. methylene blue in 8 hrs. =18 grs. in 24 hrs.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	†	†	†	†	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
A.II. 30 grs. salol in 8 hours = 90 grs. in 24 hours.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
A.III. 30 grs. boric acid in 8 hours = 90 grs. in 24 hrs.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Coll.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
A.IV. 30 m. wintergreen oil in 8 hrs. = 90 m. in 24 hrs.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
A.V. 12 grs. saccharine in 8 hours = 36 in 24 hours.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	x	xxxx	xxxx	xxxx	xxxx
B.V. Same as above	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
A.VI. 15 grs. benzoate of soda in 8 hrs. = 45 grs. in 24 hrs.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	o	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xx	xxxx	xxxx	xxxx	xxxx
B.VI. Same as above	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	x	†	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
A.VII. 50 m. oil of eucalyptus in 8 hrs.=150 in 24 hrs.	Sta. p. a.	xxxx	xxxx	xxxx	†	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	†	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	x	†	xxxx	xxxx	xxxx	xxxx
B.VII. Same as above	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	o	†	xxxx	xxxx	xxxx	xxxx
A.VIII. 20 m. sandal wood oil in 8 hrs. = 60 m. in 24 hrs.	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	†	†	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
B.VIII. Same as above	Sta. p. a.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx
	Strep.	xxxx	xxxx	o	†	xxxx	xxxx	xxxx	xxxx
	Coli.	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx	xxxx

Observations on Samples of Glycerin and Vaseline.

One cubic centimetre introduced into gelatin yielded in one case a single colony of bacteria of a common air species, in another case two colonies. It seems most likely that these gained access to the gelatin during the manipulations necessary to prepare the mixture. The cultures were kept under observations for ten days.

[1] A only tabulated unless B differs in result.

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Agar tubes were inoculated with small quantities of vaseline and placed in the incubator at 36° C. for several days. No growth resulted.

It appears, therefore, that the glycerin and vaseline were probably free from viable bacteria.

It will be noticed that in these numerous experiments clinical conditions have been imitated. In Group I. recent infection is simulated—*i. e.*, inoculation, the germ having been carried in by an instrument and lying free in the bladder, exposed to the most advantageous attack by the antiseptic. In Group II. the tissue involved in chronic inflammation is imitated, one layer of germs protects another. In actual life the tissue elements and tenacious mucus form a much better protective cover for the germs than could be well imitated in the laboratory. And it turns out scientifically, as it is proved to be practically, that the same reagent perfectly effective in Group I. is practically inert against the same germs in Group II.

This explains why by local treatment alone we may from the first avoid producing local suppuration, yet we cannot arrest local suppuration once started except by long-repeated effort, sometimes not at all. A glance at the first table also shows that while argentic nitrate is much more reliable than anything else in Group I.—bichloride is more reliable in Group II.—as might *a priori* have been inferred. The argentic nitrate coagulates the surface and protects the deeper germs, the bichloride solution has more penetrating power, and is, therefore, the more reliable the deeper the germ is situated.

The tables show mercuric bichloride up to 1 : 4000 to be absolutely effective (apparent deviations being doubtless due to errors of method and contamination) in Group I., while nothing less than 1 : 1000 can be depended upon, in Group II., where the micro organism has half a chance, and buried among the cells of a chronically inflamed mucous membrane, the micro-organism surely has much more than half a chance.

Argentic nitrate up to 1 : 2000 is fully effective in Group I., entirely unreliable in Group II. In a comparative test nitrate of silver 1 : 1000 is better than mercuric bichloride 1 : 4000.

Boric acid is worthless in both groups, and so is salt up to a 30 per cent. solution.

Salicylic acid one-half grain to the ounce, Thiersch's solution, Marchand's solution, 1 part to 3 of water, are all reliable in Group I., useless in Group II.

Some recent experiments by Ali Krogius and Sam Chedenius go to show that the staphylococcus resists mercurial preparations longer than streptococcus or coli commune ; the latter resists nitrate of silver the longest, streptococcus the least of the three.

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The medicated urines were experimented with as follows :

Two apparently healthy persons having clear urine took the same drug at the same hours on a given day. The next morning each urinated into a bottle without precautions, and the specimens marked A, B, etc., were sent for examination. They also were tested with the micro-organisms in Groups I. and II., just as the medical solutions had been tested. The substances used, with the doses taken and the hours were as follows :

A and B. Unmedicated urines.

A. I. and B. I. 2 grains methylene-blue at 2, 6, and 10 P.M., equivalent to 12 grains a day.

A. II. and B. II. 10 grains salol at 2, 6, and 10 P.M. (℥j in twenty-four hours).

A. III. and B. III. 10 grains boric acid at 2, 6, and 10 P.M. (℥j in twenty-four hours).

A. IV. and B. IV. 10 minims wintergreen oil at 2, 6, and 10 P.M. (℥j in twenty-four hours).

A. V. and B. V. 4 grains saccharine at 2, 6, and 10 P.M. (24 grains in twenty-four hours).

A. VI. and B. VI. 5 grains benzoate of soda at 2, 6, and 10 P.M. (℥ss in twenty-four hours).

A. VII. and B. VII. 10 minims Sanders' oil of eucalyptus at 2, 4, 6, 8, and 10 P.M. (Lydston), 100 minims in twenty-four hours.

A. VIII. and B. VIII. 10 minims sandal-wood oil at 6 and 10 P.M. (40 minims in twenty-four hours).

These substances were selected because they all, except perhaps the saccharine and the oil of eucalyptus, about which I am ignorant as yet, have undoubtedly a positive influence in moderating suppuration in chronic cases of kidney and bladder disease if properly selected, yet none of them cure, unless perhaps, when persistently used in moderate cases. In profound chronic disorder they are next to valueless.

As was to have been expected, therefore, these substances do not exhibit in the laboratory any influence upon the growth of micro-organisms inoculated upon them and properly cultivated. The germ grows just as rapidly as in the urine of the same individuals not medicated when treated in the same way. Group I. and Group II. are alike negative. There is a faint show of beneficial influence (as to Group I.) in the urines medicated by benzoate of soda, by oil of eucalyptus, and by sandal-wood, but it is very slight. I did not test naphthaline, extolled by Couremenos, as the dose required is impossible in most kidney cases.

A further set of experiments, too elaborate for detail here, were made by Dr. Dunham with ½ per cent., 1 per cent. and 2 per cent., solutions of phenol-sulphate of potassium—the salt which appears in the urine when salol is taken by the mouth. The result demonstrated clearly

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that while even a 2 per cent. solution did not kill those pathogenic bacteria which are the subjects of this study, yet even a ½ per cent. solution showed a marked, though not very great inhibitive, action on their growth.

I cannot terminate this study without here laying emphatic stress upon, and calling especial attention to, the value of abundantly flushing the urinary passages with bland mineral waters of the diluent variety, as a means of flooding out both pus cells and micro-organisms mechanically in many forms of chronic suppurative disease all along the urinary tract.

I know of no one single agent that possesses so much power for good. The enemy is decimated by this method of warfare and can be taken at a disadvantage. I refrain from speaking the names of the mineral springs that possess this power—and there are many of them—because the commercial spirit of to-day prostitutes such utterances.

The character of the mineral ingredient in the spring is not material. All that is required is that one may be able to stomach the water freely—to drink a gallon, two gallons a day, or more, and that the water shall not affect the bowels.

A number of such waters exist. Distilled water does not do the work, nor does rain-water. Ordinary clear spring-water fails. In selecting a mineral water it is only necessary to find a cheap one that may be consumed in vast abundance without a feeling of repletion—one that will pass rapidly through the kidney, having no influence upon the individual other than the diuretic one.

And in closing, I wish further to emphasize two prognostic points: one is the common error of believing that the presence of pus in the urine is capable of accounting for a considerable amount of albumin. One constantly hears this expression: "Albumin in the urine accounted for by the pus." Surely this is true of a trace of albumin—but only of a trace. An admixture of blood with the pus, or of blood-serum coming from an excoriated tumor in the bladder, or from an ulcer; this will account for albumin in quantity; but when there is no tumor, no stone, no ulcer in the bladder, there may be very considerable amounts of pus with barely a trace of albumin, and if under such physical conditions the amount of albumin be relatively large, it usually means pyelitis—implication of the kidney in interstitial or parenchymatous change, and by so much becomes a factor of importance in prognosis. Anything over ½ or 1 per cent. by *weight* of albumin in urine, however purulent—if there be no blood in the specimen, no tumor, no stone, no ulcer in the bladder—becomes a factor in the question of prognosis when the point of the expediency of an

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operation comes to be considered. These cases may be assumed to have already reached a degenerative stage, and the chances of lighting up surgical kidney by operative interference below are relatively great.

The prognosis is, however, not worse—not so bad—as it is in the case of sclerotic kidney, with dilated ureter, etc., those cases in which there is active polyuria, worse at night, clear or nearly clear urine, with only a faint trace of albumin, or none at all. Such patients, after operation, are apt to die intensely uræmic, passing quarts of urine, and sometimes with little or no fever.

I may briefly summarize my conclusions as follows:

1. Healthy urine is sterile.

2. Purulent urine is always microbic.

3. Microbian infection takes place from within the body by a number of methods in the course of disease; it is often brought about by instrumental manœuvres on the part of the surgeon.

4. A healthy organism and vigorous bladder may cope successfully with microbic invasion, and rid itself spontaneously, or with a little aid, of all damage arising therefrom—showing little or even no inflammatory response.

5. A suitable condition of the patient's soil is essential to the propagation and perpetuation of inflammatory phenomena upon the urinary tract—after microbic invasion.

6. This condition, intensified by traumatism and physical weakness, notably of the degenerative variety, is most intense when there is vesical distention with atony, and when the ureters are dilated and the kidneys involved in the changes incident to tension below—namely, atrophy and sclerosis above, with or without surface catarrh.

7. Under these circumstances surgical pyelonephritis is most likely to declare itself as a result of microbic infection from below (occasionally from above)—in the course of suppurative disease or after operative interference.

8. Asepsis, antisepsis, and sterilization of urine are ends to be aimed at in genito-urinary surgery—but, like all other greatest goods, not yet attained in perfection. Much, however, can be done by local means in a prophylactic and curative way, little by internal medication, and possibly as much or more than by any other means by flushing the urinary passages with natural mineral waters.

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THE BACTERIOLOGY OF PYELONEPHRITIS.

BY GEORGE M. STERNBERG, M.D., LL.D.,

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In considering the etiology of nephritis a broad distinction must be made between those forms known under the general name of Bright's disease and pyelonephritis, or the so-called surgical kidney. In the first-mentioned forms, which may be denominated hæmatogenous, the primary cause of the renal inflammation is, as a rule, and perhaps always, some toxic substance in the blood which acts as an irritant to some portion of the renal parenchyma. And when micro-organisms are found in the urine in cases of this kind or in sections of the kidney removed post-mortem, their presence is probably to be regarded either as accidental or as secondary to the changes in the kidney, rather than a cause of these changes. It is not at all unusual to find colonies of micrococci or of bacilli in the necrotic foci found in certain forms of nephritis, but the inference that the inflammatory process and resulting necrosis were due to these bacteria is not justified either by the microscopic appearances of the secctions or by experimental investigations. On the other hand, in ascending nephritis or pyelonephritis, which is very commonly secondary to a cystitis of long standing, there is good reason to believe that the inflammatory changes and pus formation depend principally upon the presence of certain bacteria which are found in the urine of such patients during life and in the diseased kidney removed by surgical operation or post-mortem. And recent researches show that the bacillus coli communis, which is constantly present in the intestine of healthy individuals, is found more frequently than any other micro-organism in the so-called "surgical kidney." This bacillus is now known to be the usual cause of peritonitis. It has been obtained, in pure culture, from a number of cases (8) of abscess of the liver,* from urinary abscesses (6),† and from the pleural cavity in certain cases of pleurisy (9).‡

The most important and comprehensive work upon the bacteriology of pyelonephritis is that of Schmidt and Aschoff,§ published in Jena

* Bignami, Stern, Netter and Martha, Rodet and Roux, Létienne, Veillon and Jayle, Vivaldi, Levy.

† Krogius, Clado, Albarran and Hallé, Tuffier and Albarran.

‡ Albarran and Hallé, Sevestre, Renard, Lesage and Macaigne, Levy.

§ Die Pyelonephritis in anatomischer und bacteriologischer Beziehung, und die ursächliche Bedeutung des Bacterium coli commune für die Erkrankung des Harnwege.

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in 1893. The authors named give a complete *résumé* of the literature of the subject, and a full report of fourteen cases of pyelonephritis, in which they have made bacteriological investigations. They also report a series of experiments upon rabbits, in which injections of a pure culture of the bacillus coli communis were made into the left ureter, after tying it below the point of injection. The ligature was removed after the injection had been made and the wound in the abdominal wall, which had been made with antiseptic precautions, was closed. Some of the animals so treated died in from twelve hours to four or five days, while others survived and were killed on the seventh and ninth day.

The left kidney, especially in the cases which survived the operation for several days, was found to be two or three times as large as the right and to present all the evidences of parenchymatous inflammation. The pelvis of the kidney contained more or less ammoniacal urine, pus, and bacilli; the parenchyma gave evidence of diffuse inflammation and contained numerous bacilli. As a rule a pure culture of the bacillus coli communis was obtained from the inflamed kidney.

A similar experiment was made with a species of proteus (vulgaris?), and with a similar result. The animal died at the end of two days. The left kidney was twice as large as the right, the surface of a deep-red color dotted with numerous white spots; the parenchyma had a striped appearance on section and a greenish color in the vicinity of the pelvis, which contained ammoniacal and bloody urine. A putrefactive odor was given off from the organ. Proteus in pure culture was recovered from the interior of the kidney.

Another rabbit received in the same way an injection into the ureter of a bouillon culture of staphylococcus pyogenes aureus. The animal died at the end of forty-eight hours. A fibrinous inflammation was found at the point of injection and the left kidney was about twice as large as the right. The capsule was easily detached and the surface was of a deep-red color dotted with small white foci of infection, which also existed throughout the organ. The pelvis contained thick reddish-colored pus. Staphylococcus aureus was recovered in cultures from the kidney and from blood taken from the heart.

Schmidt and Aschoff say that the changes found in the kidneys of rabbits after the injection of bacillus coli communis into the ligated ureter correspond with those seen in the "surgical kidney" of man. They were surprised at the rapidity with which the bacilli penetrated the urinary tubules. The first changes in the parenchyma of the organ occurred at the end of thirty-six hours, and at the end of

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five to seven days these changes had reached their extreme development. They evidently depended upon the invasion of the urinary tubules by bacilli. This conclusion corresponds with that reached in previous researches by Albarran,* Achard and Renault,† and by Krogius.‡ According to Schmidt and Aschoff, some of the earlier observers have described non-liquefying bacteria obtained from the bladder in cases of chronic cystitis or of pyelonephritis following cystitis, which correspond in morphological and biological characters with bacillus coli communis, and were no doubt identical with it. They believe that the bacillus described by Clado § (1887) under the name of "Bactérie septique," and subsequently found by Albarran and Hallé ‖ in forty-seven out of fifty cases of cystitis (fifteen times in pure culture), and called by them "Bacille pyogène," is in fact the bacillus coli communis.

The elaborate researches of Rovsing¶ upon the bacteriology of chronic cystitis were published in 1890. Thirty cases were studied, with the following results: In one case diagnosticated as cystitis no bacteria were found; in three cases culture experiments gave a negative result, but the tubercle bacillus was found in the urine by microscopical examination—in these cases the urine was strongly acid; in twenty-six cases the urine was ammoniacal, and in all of these bacteria were found—usually but a single species. All of these grew in the usual culture media except the tubercle bacillus, which in two cases was associated with some other species, and all produced alkaline fermentation in sterile urine when added to it in pure cultures. The following species were found: Tubercle bacillus, staphylococcus pyogenes aureus, staphylococcus pyogenes albus, staphylococcus pyogenes citreus, streptococcus pyogenes ureæ (n. sp.), diplococcus pyogenes ureæ (n. sp.), cocobacillus pyogenes ureæ (n. sp.), micrococcus pyogenes ureæ flavus (n. sp.), diplococcus ureæ trifoliatus (n. sp.), streptococcus ureæ rugosus (n. sp.), diplococcus ureæ (n. sp.), cocobacteria ureæ (n. sp.).

Pure cultures of all of these species introduced into the bladder of rabbits failed to induce cystitis, even when injected in considerable

* Thèse de Paris, 1889.

† Sur les rapports du b. coli comm. avec les b. des infections urinaires. Soc. de Biol., 1891, p. 830.

‡ Recherches bactériologiques sur l'infection urinaires. Helsingfors, 1892.

§ Deux nouveaux bacilles isolés dans les urines pathologiques. Soc. Anat., May, 1887, p. 339.

‖ Note sur une bactérie pyogène et sur son rôle dans l'infection urinaire. Bull. de l'Acad. de Méd. T. xx., 3e S., Paris, 1888, p. 310.

¶ Die Blasenentzündungen, etc. Berlin, 1890.

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quantities. But when retention of urine was effected artificially for six to twelve hours, allowing time for ammoniacal fermentation to occur, cystitis was developed. When the pyogenic species were introduced under these circumstances, a suppurative inflammation of the mucous membrane occurred; the non-pyogenic species caused a catarrhal cystitis. Rovsing records the important fact, as bearing upon the etiology of cystitis, that in twenty of the cases examined the bladder had been invaded by the finger or by instruments prior to the development of cystitis.

Lundstrom (1890) isolated from alkaline urine obtained from patients with cystitis two species of staphylococci—staphylococcus ureæ candidus and staphylococcus ureæ liquefaciens; from albuminous, acid urine he obtained streptococcus pyogenes.* Krogius obtained from the urine of individuals suffering from cystitis a bacillus which he calls urobacillus liquefaciens septicus. Schnitzler† (1890) found the same bacillus, or one very similar to it, in thirteen out of twenty cases of purulent cystitis examined by him. In eight of these cases it was obtained from the urine in pure cultures, and in five it was associated with other bacteria. In twelve of these twenty cases the cystitis resulted directly from catheterization; in the others it occurred in individuals suffering from stricture or from calculus.

When cultures of this bacillus were injected into a vein in rabbits, the animals died in from three to eight days, and in every instance an intense nephritis was observed at the autopsy—twice with the formation of small abscesses. The bacillus was found in the blood and the organs generally. Injections into the bladder of rabbits almost always gave rise to a severe purulent cystitis—large rabbits were selected, and great care taken not to injure the mucous membrane of the bladder. Schnitzler was not able to induce cystitis in rabbits by injecting in the same way considerable quantities of a culture of staphylococcus pyogenes aureus.

According to Schmidt and Aschoff subsequent researches indicate that some of the species described by Rovsing as being new are in fact varieties of bacillus coli communis.

The identification of the bacillus pyogenes of Albarran and Hallé (Bactérie septique of Clado) with bacillus coli communis was first made by Krogius‡ (1891), and about the same time by Achard and Renault.§

* Sur un bacille pathogène trouvé dans les urines pathologiques. La Semaine Méd., 1890, No. 31.

† Zur Aetiologie der akuten Cystitis. Centralbl. für Bakteriol., Bd. viii., 1890, p. 789.

‡ Note sur le rôle du b. coli commune dans l'infection urinaire. Archiv de Méd. expér., t. iv., 1892, p. 66.

§ Sur les rapports du b. coli commune avec le b. pyogènes des infections urinaires. Soc. de Biol., Dec. 12, 1891, p. 830.

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In twelve cases of cystitis, six of which were complicated with ascending nephritis, Krogius demonstrated the presence of bacillus coli communis, and showed that in its growth in culture media it corresponded with the bacillus pyogenes of previous authors. In a second communication Krogius* states that in twenty-two cases of cystitis studied by him he obtained bacillus coli communis sixteen times, and of these fourteen times in pure culture. He also calls attention to the fact that in those cases where no other micro-organism was associated with the colon bacillus the urine was always acid—a statement which is sustained by the subsequent researches of Schmidt and Aschoff. He also gives details with reference to the pleomorphism of this bacillus and differences in the appearance of gelatin cultures from different sources, the growth being sometimes opaque and sometimes transparent. When cultures of this bacillus were injected into the bladder of rabbits and retained by ligating the urethra, an intense cystitis was developed in from twenty to thirty hours. Injections into the ureter gave a result similar to that subsequently reported by Schmidt and Aschoff. The animal died in about two days, and pyelitis, together with more or less necrosis of the renal epithelium, was found at the autopsy. Reblaub† (1892) obtained bacillus coli communis in pure culture in six out of sixteen cases of cystitis examined.

In their latest publication Achard and Renault‡ arrive at the conclusion that there are some differences between their "urobacillus" and bacillus coli communis, which they state as follows:

1. Upon most media, especially upon malt-agar, the growth is more luxuriant.

2. Cultures of the urobacillus upon potato appear grayish-white, very luxuriant, and have many gas-bubbles.

3. The urobacillus develops much gas even in gelatin and agar cultures containing little sugar.

Morelle§ (1892) and Denys‖ (1892) in their bacteriological researches obtained from numerous cases of cystitis a bacillus which they identified with bacillus lactis aërogenes of Escherich. But the last-mentioned author has since stated that this bacillus presents varieties which cannot be distinguished from the typical cultures of bacillus coli communis.

* Recherches bactériologiques sur l'infection urinaire. Helsingfors, 1892.

† Des cystitis non-tuberculeuses chez la femme. Paris, 1892.

‡ Sur les bacilles de l'infection urinaire. Compt. rend. de la Soc. de Biol., April 9, 1892, p. 311.

§ Étude bactériologique sur les cystitis. Lierre, 1892.

‖ Étude sur les infections urinaires. Bull de l'Acad. Royale de Méd., Louvain, 1892.

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The recent researches referred to having shown that bacillus coli communis is very commonly present in the urine in cases of cystitis, and often in pure cultures, its etiological relation to the disease in question seems probable, and this view is further sustained by experiments upon animals and by the demonstrated fact that retention of urine *per se* does not give rise to inflammation of the bladder. But this is not the only micro-organism which is capable of causing a cystitis when introduced into a bladder which has suffered some kind of mechanical injury or has been subjected to the action of chemical irritants contained in the urine. The researches of Krogius, Schnitzler, and of Schmidt and Aschoff show that next to the colon bacillus the micro-organisms most commonly found in cases of cystitis and of pyelonephritis is a proteus (P. vulgaris?).

At the date of the publication of the monograph of Schmidt and Aschoff the bacillus coli communis had been found in pure culture in sixty cases of cystitis, and the proteus in thirteen cases. The authors named say :

"But two species of bacteria (bacillus coli communis and proteus) have, up to the present time, been demonstrated to be the cause of pyelonephritis. When, in addition to these, other bacteria or cocci are found, we have a pyæmic mixed infection."

An important point to be kept in view is the fact that when bacillus coli communis is found in the urine in pure culture, this fluid is more or less acid, as the bacillus in question does not give rise to alkaline fermentation, at least not under the conditions found in the bladder and in the absence of retention. But when proteus is present the urine is almost always ammoniacal.

The number of cases of pyelonephritis reported since 1889 by the authors heretofore referred to (Albarran, Rodet, Morelle, Krogius, Achard and Renault, Schnitzler) in which bacillus coli communis was found and was probably the exciting cause of the ascending nephritis, is twenty-nine, and in twenty of these the bacillus referred to was found in pure culture.

Finally, I think we are justified in concluding that cystitis and ascending pyelonephritis are usually caused by micro-organisms introduced through the urethra into a bladder which is rendered susceptible to infection by mechanical violence or chemical irritation. And that the most frequent cause of such local infection is the bacillus coli communis, which is constantly present in the intestine and upon the external surface in the vicinity of the anus, from whence it may easily be transported to the interior of the bladder by catheters, etc., used by the patients themselves or by their medical attendants. Accord-

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ing to Bouchard* it has been shown that this bacillus is sometimes found under the prepuce and about the vulva of healthy persons, and this is what we should expect from their proximity to surfaces which are constantly soiled with discharges containing it. But there is no evidence that the bladder is reached by the direct invasion of this or other bacteria without mechanical assistance. The researches of Lustgarten and of Mannaberg,† and of Krogius‡ show that bacillus coli communis is not found in the normal urethra.

Dr. George Chismore, of San Francisco.

Having no previous knowledge of the subject matter of the papers read I feel the greatest diffidence in offering any remarks. I have also felt in listening to them that where the conclusions drawn from my own experience agree with those of the author, I shall repose in them a strengthened confidence and that in such points as my own conclusions differ, I shall have to again go over the ground carefully.

I think it only right however, that I should express some differences that have occurred to me. In reference to nephritis due to stone in the bladder, instead of stone in the bladder being so frequently a cause of nephritis and rendering instrumentation likely to set up ascending nephritis, it has seemed to me that under certain circumstances the reverse has been the case, and I believe that stone in the bladder brings about a tolerance and lessens the resentfulness to instrumentation. I have accounted this due to the same principle that explains tolerance of the urethra induced by repeated instrumentation.

In regard to the intolerance of the bladder and the consequent liability to ascending nephritis in the young, my experience has also differed. It seems to me that the bladder is more resentful and that the introduction of an instrument for any purpose is more liable to be followed by renal mischief, particularly of an acute inflammatory character in the young than in old and especially those with stone.

With regard to the presence of a low specific gravity with polyuria, I fully agree that such a condition of affairs makes a man more dangerous to touch in the way of instrumental interference with the bladder or ureter, and the danger lies in the occurrence of ascending nephritis.

* Charrin: Sur la bactérie commune des infections urinaires. Soc. de Biol., 1891, p. 851.

† Ueber die Mikro-organismen der normalen männlichen Urethra. Vierteljahrsschr. f. Dermatol. u. Syph., 1887, No. 4.

‡ Arch. de Méd. expér., 1892, p. 75.

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In regard to attacking the microbes by the method of injections into the bladder it seems to me that the process is a faulty one in that you reach only a portion of the genito-urinary tract, that you leave whatever microbes may exist in the ureter and kidney untouched and that there is the danger of over-distension and of infection by the process and that it is not devoid of bad results. It seems to me that we can not be too conservative in these cases, particularly as it is not easy to diagnose early in the process, affections of the bladder or kidney of a tubercular character. I think that now it is generally known that where there is a tubercular element, instrumentation is liable to be followed by great injury without a corresponding hope of benefit. Then too when you reflect on the toxic effects of very strong injections, I think that it is only fair to assume that in weak solutions there must be a certain proportion of toxic action. It is not only the meeting of the man and microbe that is necessary, but a certain amount of traumatism is required. I think that this traumatism may be furnished by the use of injections of extreme tenuity and by instruments which have been sterilized.

It is unnecessary to say how anxiously we look for the occurrence of surgical kidney after operation and how interested we are in any measure that will decrease the danger of that calamity. I shall however, not trespass further on your time in presenting my own ideas. I shall leave the discussion of Dr. Sternberg's remarks to abler hands. I have not had the opportunity or time to engage in observations of that character myself and I am not familiar enough with the investigations of others to attempt to criticise what he has so ably set forth.

Dr. Francis S. Watson, of Boston.

In the few minutes in which I shall ask your attention I shall not attempt to discuss the broad questions relating to this subject, such as the bacteriology, causation and nature of the disease, for they have been discussed far better than I am able to do. I shall confine myself to one point in the practical treatment of the disease. This has not been touched upon and it has received far less attention from the profession than is its due. I refer to long-continued or permanent drainage of the bladder and its beneficial effect upon pyelonephritis of the nature under discussion. We are all familiar with long-continued or permanent drainage for the relief of distressing bladder symptoms, especially such as arise in connection with senile hypertrophy of the prostate. But comparatively little has been said about its effect upon surgical kidney and comparatively few observations have been brought

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forward for our use. Some surgeons have spoken of it, notably Mr. Reginald Harrison who some years ago, called attention to this procedure and urged its adoption upon the profession for that especial purpose. He also reported a number of cases in which he had established long-continued bladder drainage, with the primary object of treating the renal disease, which had resulted well. As these observations have been few, I should like to add those of six cases occurring in my own experience in which pyelonephritis was greatly benefited by long-continued bladder drainage and in which it was undertaken with the definite object of relieving that disease, and in which in my opinion, it saved the lives of the patients. It would not be proper here to speak in detail of these cases and I wish merely to summarize some of their more important points which may serve to illustrate the value of the method. These patients were all over sixty years of age and were all subjects of senile hypertrophy of the prostate. The lives of all of them were in great danger from pyelonephritis of the existence of which there was ample evidence. Four of the cases were in the acute stage and two in the chronic stage. In four the disease had followed in a comparatively short period (a few days to a few weeks), the beginning of catheter life. In two it occurred without apparent exciting cause after several years of regular use of the catheter. Of two only can I speak from personal observation as to the existence of pyelonephritis previous to the use of the catheter. In these two, I can say that it was not present. Two of the cases I treated by continuous drainage by means of a catheter passed through the urethra, and four by means of a large soft rubber drain introduced through a perineal opening. In every one of the cases the conspicuous sign of improvement, was in that symptom of the disease which has been referred to as one of the most important, namely polyuria and low specific gravity. The first sign of improvement was increase in specific gravity and diminution of the polyuria.

I would not be understood as claiming too much for long-continued drainage. Nephrotomy in certain cases has its place and possibly nephrectomy may be of utility in certain cases, but I think that long-continued drainage of the bladder has not received the full attention in the treatment of the disease that it deserves. The class to which it is especially applicable is that of the old prostatics in whom the disease is already well marked or in whom it is evidently going to be well marked. No one need hesitate on account of the acuteness of the symptoms, fever, prostration, typhoid condition, etc., to tie in a catheter at any rate. There might be thought to be grounds for hesitation when we know that the catheter has been the exciting cause of

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the trouble. But while we may set up a pyelonephritis by the passage of a catheter we may also save life by keeping a catheter tied in and draining the bladder continuously.

As to the methods of carrying out permanent drainage, that through the meatus has the disadvantage of the liability to set up urethritis. It is much the simplest however and involves no operation. It may be done as a preceding step to the best way of all of carrying out permanent drainage, namely through a perineal opening. The patient having convalesced to a certain point while wearing a catheter through the urethra we perform perineal section and introduce a large drainage tube and the patient can go about when well enough to do so with comfort. I have a patient who has worn the perineal tube for three years with entire comfort.

Just in what way long-continued bladder drainage acts is uncertain. My belief is that it acts by direct drainage of the pelvis of the kidney as well as by cleansing the bladder. The subsidence of the cystitis and the relief to the general condition by the cessation of the pain and restoration of the bladder mucous membrane to a state of health is no doubt also a large element, taking away as it does also of just so much of the power of the original focus to infect.

Dr. Edward L. Keyes, of New York.

A philosopher has said that articulate speech was given to man in order that he might conceal his ideas. I have been successful in the case of Dr. Chismore. While I did not read all of my paper I did devote a page and a half to stating that the bladder is the guardian of the ureter. Where there is stone, as long as the bladder keeps healthy, there will be no trouble.

As to young people having ascending nephritis more often than the aged, that is a matter of opinion.

With regard to what Dr. Watson has said there is no doubt that a certain amount of benefit is obtained by the use of the catheter and by the perineal tube still more. I however did not take up this question in the paper, because the amount of material to be gone over was too extensive and I devoted myself to the scientific questions. I am however, glad to corroborate the ideas of Dr. Watson as to the justifiability of the means he mentions.

I am not a bacteriologist and relied upon one whom I considered to be an expert to use proper methods and I obtained the results as stated. I think that the observations of Dr. Sternberg are corroborative of the conclusions which I have advanced. An understanding of

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these matters will enable us more successfully to approach this condition from the prophylactic side and take from surgery the opprobium which attaches to the name of this malady, the so-called surgical kidney. It has been remarked that it was improper to call this affection "surgical" kidney because the existence of the disease was due to the physician, for if he had been a competent surgeon there would be no surgical kidney. There is no question that the disease is sometimes spontaneous. I have discussed the methods of infection. It is an ascending nephritis, rarely a descending nephritis. Occasionally it is due to traumatism inflicted by the carelessness of the physician, but more often it is due to the surgeon's interference so that perhaps on that account should still be called surgical kidney.

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THE CONSERVATIVE SURGERY OF THE FEMALE PELVIC ORGANS.

BY WILLIAM M. POLK, M.D.,

Professor of Obstetrics and Gynecology, University of the City of New York.

I AM here with a keen sense of my obligation to the American Gynecological Society and my responsibility to you, seeing that I have been appointed to open the discussion upon conservative operations in that portion of the body of the human female with whose disorders our Society chiefly concerns itself, and whose integrity is of such vital importance to our race. I am aware that I am expected to confine myself principally to the consideration of conservatism as it applies to the so-called uterine appendages, and it is my intention to comply with this expectation; but something introductory to this the kernel of my subject is needed in order that it may be clear that what I say in this relation is not strained or forced deduction, but, on the contrary, suggestion in keeping with the avowed intent and purpose of all surgical work, whether done in our special field or in other regions of the body.

There are three applications of the term conservative as used in surgery. One considers the interests of the frame as a whole, as in malignant or in the later stages of infectious disorders, and indicates a sweeping removal of tissue, regardless of the affected organ, or even its surroundings—being therefore most radical in one sense and yet most conservative in another.

A second application is to traumatism, to all non-malignant or to the early stages of infectious disorders, and indicates the retention of all tissue or organs which have a reasonable chance of adding to the local or general well-being of the patient.

The third application of the term conservatism among us is one of derision, and is employed to imply that its advocate, wanting in nerve, uses it rather as a justification of a surgical incompleteness which, is due less to the real demands of the situation than to the ignorance or timidity of the operator. It is needless to say that the last acceptation of the term is out of place in this discussion; and as there can be no issue upon the first, we have left for the purposes of this discussion only the second. We beg leave, then, to direct your attention solely to conservative operations as applicable to traumatism, to

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the non-malignant disorders, and to the earlier stages of the infectious disorder (sepsis) found in the genital tract of the female.

Brief consideration will quickly show that conservatism characterizes most, if not all, accepted operations in the regions in question, the preservation of function and utility being the primary object. Nowhere is this more strikingly shown than in the measures addressed to the perineum and vagina; for volumes have been written upon the measures deemed proper for the restoration of the perineum, and the need for the preservation of the integrity of the vagina is almost the *sine qua non* of every operation addressed to this canal.

Coming to the cervix uteri, we find among operators less unanimity upon the question of restoration than prevails for the regions below, the differences depending largely upon a denial by some of the urgent importance of this structure to the requirements of the genital canal as a whole; while, therefore, others aim always to so operate as to retain as much of the original outline of the cervix as possible, some sacrifice it with comparative impunity, troubling but little about the repair of lacerations.

Upon the whole, however, it appears that repair of the cervix is preferable to amputation, except in certain cases of hypertrophic elongation, and even here amputation is so conducted as rather to restore the structure to its natural dimensions than to excise it wholly.

Continuing our line of thought to the uterus, we find conservatism in all directions. We have measures looking to the restoration and maintenance of the carvical canal, as, for instance, the operations of incision and divulsion; others devised to correct or control malpositions, as in hysterorrhaphy and shortening the round or the utero-sacral ligament; myomectomy as applied to submucous and subserous fibromata; of late, ligation of one or several of the arteries of the uterus, with the intent of arresting hemorrhage and growth through curtailment of the blood supply;* and finally curettage, with associated measures which look not only to drainage but to depletion—to a depletion confined not merely to the uterus, but extending its beneficent influence to all the uterine surroundings or attachments.

Out of the many devoted to this organ, let us select the conservatisms of latest date, some of them still *sub judice*. I shall name three—myomectomy and ligation of the arteries of the uterus in fibroids; curettage and "tamponade" in all forms of endometritis and metritis, whether acute or chronic, whether puerperal or non-puerperal. Myomectomy as applied to submucous fibroids is an accepted and long-

* Oöphorectomy is this connection is not considered a conservative procedure, seeing that it emasculates the uterus.

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established procedure, but as applied to interstitial and subperitoneal growths it is not. This application is a dependant of recent peritoneal surgery, and such results are already obtained that it stands as a promising candidate for popular acceptation. There are certain limitations to it, however, which require notice at our hands.

All pedunculated fibroid, fibro-cystic, or myomatous growths are favorable to myomectomy, and the same may be said of all possessed of well-defined capsules, such, for instance, as the true fibroid formations; but it is evident that the number of such formations in any given uterus, and their location and size, must exercise a marked influence upon conservatism. A uterus may be so riddled with fibroids, or the tumor may be so large or have such relations to the main vascular trunks, as to nullify any attempt at conservatism, for such injury may have to be inflicted upon the organ as a whole as will render it useless either for purposes of menstruation or pregnancy. Touching the pure myomata, it may be said that in general their connections with the uterus are so broad, so little differentiated, and so vascular, myomectomy is apt to be accompanied or followed by such persistent hemorrhage as to render the process unsafe. With certain of the fibro-cystic growths the same objection may prevail. Pedunculation and fibrous encapsulation are conditions favorable to the operation, and the fewer the tumors and the smaller the pedicle the more perfect the result.

We may then formulate the indications for myomectomy as follows, enumerating the conditions in the order of efficiency:

1. It is applicable to uteri which possess only pedunculated tumors.

2. To uteri whose tumors possess well-developed fibrous capsules, and which are neither so large, so numerous, nor so placed as to require in removal such direct damage to the uterus or to its blood supply as will unfit the organ for proper functional activity.

Looking further into the problems presented by fibroid disease, we find that of late "ligation of the broad ligaments of the uterus" has been practiced as a remedy. It is designed to check growth and hemorrhage, and, judging from the few reports at hand, it appears capable of fulfilling in part, if not in whole, the expectations of its advocates. The procedure is a ligation of one or several of the arteries feeding the uterus, leaving at least one or more of these vessels, according to the exigencies of the particular case. The fact that it aims to accomplish a beneficent purpose without the dangers of a more extended operation, and that it appears to fulfill its purpose as well or better than is done by oöphorectomy, commends it to us in cases unfitted for myomectomy on the one hand or incapable of withstanding

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the radical operation on the other. Our knowledge is insufficient at present to permit us to go beyond this statement.

Of all the conservative operations upon the uterus, the simplest and the most far-reaching in its results is curettage with the drainage and depletion which is obtained by the plentiful packing of its cavity with sterilized gauze. This is a measure appropriate to all forms of endometritis and metritis, whether acute or chronic. The most striking results are those obtained in the face of the acute septic inflammations of the organ which occur after abortions or labors; but to be of proper service the measure should be promptly applied, delay affording opportunity for systemic infection, after which the treatment, though far from useless or dangerous, is of less service. This fact, however, is common to similar efforts at evacuation, drainage, and depletion when applied to other organs with peritoneal connections; the phenomena of late operations in appendicitis, in which we find conditions of local and systemic infection somewhat analogous, bear out this statement. But the negative results of late interference thus met with in these conditions, instead of forbidding such interference, merely leave the question to be settled by the results of early interference. These are overwhelmingly favorable. If any one doubts this statement we beg that in future, in the sepsis of abortion or labor, he forego his vaginal douches, with other expectant methods, and at the first sign of an outbreak boldly enter the uterus and treat it as above indicated.

The dangers which beset the chronic varieties of uterine inflammation are so much less than those just mentioned, the results to be had from an extension of the above treatment to the various forms are less striking, but nevertheless they are fully as useful, for by means of this same curettage and packing these chronic inflammations which were for so long a time a reproach to our specialty are now well within the area of complaints which are promptly and permanently curable. I expect that some may claim that in these latter complaints gauze packing is useless, that curettage suffices. This is so far contrary to my own experience that I beg to enter a protest in advance. There are few cases of chronic uterine inflammation in which depletion is not demanded. Wherever any degree of involvement of the wall is present it is a necessity, and nothing gives this better than the gauze. Pack it in place firmly and abundantly, and, apart from the drainage which it secures, it will force the uterus into contractile efforts; and in consequence every tissue in the organ is stimulated to new and increased action, the effect of such action upon all adventitious products being in the direct line of disintegration and absorption, so that when

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the packing is expelled or removed an effect has been produced which will do more than can be done by any other measure to give us a normal uterus. Reason and experience being favorable to this method, it is safe to affirm that curettage and packing with sterilized gauze is a positive advance in treatment and one that will hold a permanent place among conservative operations.

Approaching now the appendages, we near our point of resting. It is an error to suppose that these structures are amenable only to direct operations. The poor results which formerly met us in our efforts to secure resolution of inflammation in those structures are familiar to all of you. I refer to the treatment by measures confined to the vagina, aided perhaps by some applied over the hypogastrium and iliac regions, such, for instance, as the hot douchings, the tamponade of the vagina, the counter-irritations both within and without.

Indeed, it was mainly the insufficiency of such measures that gave such tremendous impetus to oöphorectomy and salpingotomy. The wave of celiotomy which swept over the civilized world obscured the possibilities which lay within the uterus. It was a long time before it was recognized that the principles of pressure and depletion involved in the vaginal tampon could be applied within the uterus, and even when applied its benefits were not promptly recognized. We know now, however, that in certain cases heretofore deemed fit only for castration sufficient depletion can be secured by way of the cavity of the uterus as to place such cases, if not wholly within the pale of absolute cure, yet sufficiently near to warrant confidence in subsequently leaving to the beneficial influence of time and proper general measures a condition of affairs which, short of such intrauterine treatment, doomed the patient to invalidism. It is idle to suppose that the method is without limitations. It belongs to the stages of infiltration, for when inflammation has passed to suppuration or organization its benefits are but slight. Therefore in the earlier phases of salpingitis and ovaritis it gives its best results. It must not, however, be inferred from this statement that the chronic phases of these disorders are beyond its influence. Whenever in chronic inflammation the uterus is yet enlarged, is yet in the stage of infiltration, this measure is applicable. This brings within its influence many of the so-called cases of recurrent salpingitis and every case where endometritis, and particularly metritis, persists. I am aware that support has been withheld from the treatment because it might lead to temporizing in the face of conditions of inflammation urgently dangerous to the patient's life, to conditions of suppuration in the tubes and ovaries which threaten speedy general peritonitis. We have already said that

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little in the way of cure, or perhaps even of amelioration, can be expected in such conditions; and while even this statement is susceptible of modification in favor of the treatment in certain cases of acute septic inflammation, we will do no more now than add in reply that, as the science of diagnosis can afford such patients ample protection, it is not worth while, in this day of medical activity and opportunity, to abandon this conservatism until such time as ignorance chooses to inform itself. The measure is after all an operation of sufficient importance to demand special knowledge, and should one know enough to be able to remove the appendages he is competent to determine its application. If it be still claimed that acceptance of the measure will lead to its inappropriate use, I reply that done properly it is harmless, and being done it will not preclude more radical measures; in fact, it aids them, first, in that it frees the uterus from disease—a consideration of the greatest importance in unilateral salpingitis, and of benefit even to the organ when wholly bereft of its appendages; second, in that by aiding resolution it may pave the way to a preservative operation upon its appendages.

I will not detain you by a dissertation upon the *modus operandi* of this treatment; your knowledge of the lymphatic and vascular connections between uterus and appendages, the relation of the canal of the tubes to the cavity of the uterus, indicates this to you; and remember that inflammation in the uterine appendages is amenable to the same influences which prevail against it elsewhere, and just as drainage and depletion affect it in other situations, so they affect it here.

My function here is to present facts bearing out the principles I advocate. Having already presented these facts in other writings, it would be tedious and useless repetition to present them here. I will simply add that the more of this kind of treatment I use the better I like its results. Women who but recently, even in the light of most careful diagnosis and sharpest differentiation, lost their ovaries, can with this measure now retain them in reasonable enjoyment of life, health, and happiness.*

Pursuing our subject upon the lines laid out, we rest at the fountain-head of all this tract—the appendages. Chief among the organs of reproduction are the ovaries, for they are the organs for which all the others are created; uterus and vagina serving but as channels of intercommunication in the mysteries of a conception impossible without them, or acting but as places of rest and growth in the development of their creations.

* See "New York Journal Gynecology and Obstetrics," Feb., 1892; May, 1892; May, 1894.

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I assume there can be little dispute over the proposition that women are the better mentally and physically for the maintenance of menstruation and ovulation as far as the period of Nature's menopause. If, therefore, the disturbances incident to pelvic inflammation can be brought under control by measures short of castration, this should be done.

We recognize the justice of this claim when we attempt relief, as we do, through measures short of celiotomy. Should we, then, deny it when in the immediate presence of the ovary?

I think not, because my experience has taught me that in many cases we can so operate as to free the patient from the dangers and trials of her condition, permitting her to retain menstruation, etc., and have a degree of comfort and efficiency quite equal to that found in the best cases of oöphorectomy and far superior to that found in the majority of such cases.

The time allotted this paper forbids me to consume it unnecessarily. I must, therefore, assume your entire familiarity with every expression of inflammation in the appendages. These, roughly speaking, may be enumerated for the tubes as follows: Catarrhal and parenchymatous salpingitis, pyosalpinx, hydrosalpinx; for the ovaries, periovaritis, ovaritis, ovarian abscess or disseminated purulent infiltration, and cysts of the ovary. To these must be added, as a more or less constant associate, adhesions, and finally loculi of pus within the peritoneum.

I think it is not overstating the proposition when I affirm that until recently each one of these conditions was considered adequate cause for the removal of the appendages. But, as some of you know, this proposition has never been accepted in whole by quite a number of gynecologists. Those who stood in opposition were divided between two camps. In one we found those who held to the belief that general treatment associated with local measures confined to the vagina would suffice; in the other, those who held that while laparatomy might be necessary, the laparatomy was but a means of reaching these structures in order that measures might be applied which looked less to their removal than to their retention. Both thus pursued the same end; one merely ventured more to attain it, and in attaining it was merely contending for the extension of principles to the appendages which had been found serviceable elsewhere. Outstripping their associates, these gynecologists now joined issue with their more radical brethren in their own chosen field, and here the issue is to be fought to a finish.

We recognize that the burden of proof rests upon us, but, taking a

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leaf from the book of our worthy critics, we propose to establish the truth of our contention upon the same lines followed by them when first they fought for approval. The object of their work and of ours was and is the promotion of the well-being of the patient. If, then, it can be shown that we do this as well as they, our fight is won.

First, as to what has gone before. If you will look back upon the cases of oöphorectomy reported in times gone by, you will find frequent mention of cases operated upon solely for catarrhal salpingitis or for adhesions. Questioning such operators, we have found that the term catarrhal referred in most cases to tubes with open fimbriæ, injected from recent mild inflammation and free from purulent accumulation. Again, the term adhesions covered instances of a few friable attachments, a condition added, perhaps, to the catarrhal state mentioned. As for the ovaries in such cases, they were in general so little involved as to make it certain that the ordinary processes of resolution would insure their restoration. Am I wrong in saying that conservatism has triumphed in this field? Again, how is it upon the question of removal of both sets of appendages, but one being diseased? In the light of results being obtained all about us through the improved methods of intrauterine treatment for such cases, am I saying too much when I affirm that here conservatism has scored another triumph? In the absence of reinfection of the uterus—an accident certainly preventable—I am sure that it has.

Conservatism might rest content for the present upon two such victories, but the interests of our science call for further aggressions. We propose, then, to enter the domain of destructive inflammations.

The burden of contention here is the retention of ovulation and menstruation, the question of pregnancy being for the present a side issue. Conservatism stands upon the broad proposition that this function should be preserved to the woman when it can be done without sacrifice of her health and life.

In the face of destructive inflammation of the appendages how can this be done?

All things considered, the interests of this work will be best served if we operate soon after rather than during active inflammatory action. We then secure in our behalf the resources of resolution. The exceptions to this rule are presented wherever life is threatened either through sepsis or the extension of the inflammation, when, in accordance with the proposition, this conservatism must yield. We know, however, that the general tendency of these inflammations is in the direction of resolution, and this resolution can be hastened by the intrauterine treatment before mentioned.

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I take it for granted that our experience tallies thus far. There are many cases of occluded, adherent, atrophied tubes in which the ovary, aside from some periovaritis, is in good condition. The same statement applies to some cases of pyosalpinx, to many cases of hematosalpinx, and to some of hydrosalpinx. The proposition is to remove such tubes and leave such ovaries.

To the query "Cui bono?" the reply is, "To retain the influence of ovulation and menstruation."

Naturally the nearer such patients are to Nature's menopause the less need for such consideration; but in the early or middle stages of menstrual life it is an urgent necessity. The manner of removing the tube differs from that observed in the usual amputation, for here the tube is not ligated, ligation being confined to bleeding vessels.*

Within the past four years I have operated as just indicated upon forty-five patients, and these patients, in their ability to work, to enjoy life, and in their freedom from bodily ailment, are in as good condition as any forty-five cases of oöphorectomy that I know of either among my own patients or among those of any other operator, and mentally they are better. The best of these forty-five cases are in better bodily and mental condition than the best of any similar series of oöphorectomies that I know of.

Pursuing this subject further, we come to the question of resecting ovaries which are the seat of simple or blood cysts. In practice we meet with two classes of simple cysts. In one we have one, two, or at most three cysts, varying in size from that of the ordinary corpora lutea to a dimension several times as great, the entire structure varying in dimensions from that of an English walnut to that of an average orange. In the other class we have many small cysts scattered indiscriminately throughout the cortex of the organ.

In dealing with the first class of these cases and with hematoma I have contented myself with free enucleation, bringing together the cut edges of the bed of the cyst with catgut or silk. In the second class I have resorted to ignipuncture of all cysts that could be reached, even laying open the ovary by one free incision to get access to cysts buried in the parenchyma.

I have operated upon thirty-six patients in this manner, and in all but five the results so far have been entirely satisfactory to the patients and to myself. Failure in these five was due as follows: a fresh development occurred in two; in three suppuration occurred in the cyst bed, due to infection from the faulty catgut used.

A final word must now be said upon the subject of tubes whose

* See Trans. Am. Gyn. Society, 1893, p. 181.

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canal is intact, and whose ostium abdominale being open, presumably both are. Such conditions are not very common in salpingitis, but they exist and usually are associated with adhesions.

Presupposing that the ovary or a part of it can be left, my rule has been to free such tubes from imprisonment and leave them. This I have done in twenty-eight cases with good results, treating always the associated endometritis by tamponade. Every case has done well save one, and here subsequent gonorrheal infection spoiled the work.

Such tubes as I have just mentioned sometimes present themselves with closed or partly closed ostium abdominale, and contain but little muco-purulent secretion, the conditions indicating that closure is of recent date. I have opened such tubes at the ostium, cleansed them, and after stitching back the fimbriated rim, thus giving a free opening, have left them. This has been done in twelve cases.

The question of adhesions as enforcing ablation of the appendages has been subordinated to the condition of the tubes and ovaries, for where these seemed fit for preservation the adhesions have not been permitted to contraindicate conservatism.

Such, in brief, are the conditions to which conservatism has been applied. All are of deepest interest, and the measures of relief advocated are no less so. For each I ask your consideration, but especially for those addressed to the appendages. As you see I have reported, of these, one hundred and twenty-one cases, there being three deaths; these deaths, however, depending upon causes common to all peritoneal operations.

In viewing the propositions in this paper which relate to the appendages, let me again direct your attention to the end chiefly aimed at. This relates less to the question of pregnancy than to that of ovulation and menstruation, concerning itself far more with the mental and physical aspects of the enforced menopause than with the question of procreation.

Before closing this paper, however, a word is not amiss as to the possibilities of pregnancy in the face of some of the conditions produced by the conservatism suggested. Drawing solely upon the experience of some of the members of our own Society, I will submit the following abstracts of cases already reported.

Case I.—Mild parenchymatous and endosalpingitis involving both tubes; ostium abdominale open; hematoma of right ovary; abundant friable adhesions imprisoning both sets of appendages. Right appendage removed; left freed from adhesions. Patient became pregnant ten months after operation.

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Cases II. and III. presented conditions similar to Case 1, Case 2 presenting in addition, however, a retroverted adherent uterus, which was treated by Alexander's operation supplemental to the liberation of the appendages. This case is now pregnant for the third time. Case 3, like Case 1, has reported but one pregnancy.

Case IV. is an instance of repeated abortions, traceable to the pressure effects of a small dermoid cyst of the left ovary. The tubes here were normal; the right ovary was treble its normal size. The cyst and accompanying tube were removed. The condition of the right ovary suggested extensive disease. Pregnancy, for property and other reasons, was an urgent necessity, and as the operation had been suggested and been submitted to for the furtherance of that object, the preservation of ovarian tissue was necessary. The ovary was consequently laid open down to the hilum; it was then seen to be merely the seat of extensive congestion and infiltration. The incision was closed and the organ was returned. The patient soon became pregnant, and at this date is within three weeks of her confinement.

Case V. presented much the same reasons for pregnancy as Case 4. She had aborted three or four times, at the sixth and eighth weeks. Both ovaries were enlarged, and evidently by their pressure effects caused the abortions. In order to promote pregnancy operation was suggested and accepted.

The left ovary was practically destroyed by an extensive hematoma; the whole mass, including the ovary, was about two inches in diameter. This, with the associated tube, which was normal, was removed. The right tube was nearly normal, but its associated ovary was the seat of a small hematoma. Scattered over the face of this set of appendages were a few friable adhesions.

About two-thirds of the ovary was cut away. The edges of the incision were then drawn together with catgut; the tube and what remained of the ovary were then replaced in the pelvis. This patient gave birth to a child ten months after this operation.

Case VI. represents a woman who had also been the victim of frequent abortions in the first months of pregnancy, the pressure effects of enlarged adherent ovaries being as in Cases 4 and 5 responsible for these accidents. The right ovary, greatly enlarged, was the seat of an extensive hematoma; the left was normal, but the corresponding tube had become converted into a hematosalpinx. The appendages on the right were removed. The dilated portion of the left tube was cut off, leaving about one inch of the tube upon this side, together with the ovary, which, as has been said, was normal (excepting the remnants of the adhesions which were removed).

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About two years after this operation the patient conceived, and at the proper time was delivered of a child. She has recently been again pregnant, but, wearying of maternity, secured a criminal abortion.

The above six cases have all occurred in the writer's practice, and each one of the patients is in good health.

Case VII. occurred in the practice of our Associate Fellow, Dr. Baer, and is reported by him in the *Annals of Gynecology and Pediatrics*, January, 1894, pages 231 and 232.

In this case the uterus and appendages appear to have been embedded in firm adhesions. Those upon the left were removed; those upon the right, after having been dissected from their "pathological attachments, were in shreds, the tube having been torn off about two inches from the uterus." At the suggestion of a bystander this ovary and remnant of a tube were permitted to remain. Fifteen months later the patient gave birth to a child.

Case VIII. occurred in the practice of our Associate Fellow Dr. McMonagle. I have been granted permission to present the case in connection with this paper, and, as it has not been heretofore published and is of such striking interest, I present it from the doctor's notes:

Mrs. M. S. H., American, aged 25 years; menstruated at 14 years; married three years; never pregnant; very anxious to have a child. Consulted gynecologist, who did gradual dilatation of the cervix uteri every other day for two weeks. This was very painful, and at the last dilatation pain became very severe in left lower abdomen.

Was in bed, with nausea, vomiting, great pain, distention of abdomen, fever, etc., for six weeks, when there was a discharge of pus from the rectum, followed by great relief. During the next six months was part of the time in bed and suffering, always relieved by discharge of pus from rectum.

October, 1888, came to me. I found her very thin; night sweats; sallow skin; poor digestion; bowels constipated; frequent desire to pass urine; some tenesmus; abdomen flat, scarred by blisters and poultices; tender in both iliac regions; heart and lungs normal; urine normal; temperature 99° to 101°; bad-smelling, yellowish discharge from vagina and uterus; pus and blood in stool every few days; uterus fixed; thickening and tenderness over both ovaries and Fallopian tubes.

Opened the abdomen by three and one half inch incision. Found omentum adherent to uterus and surrounding parts; small cyst over right Fallopian tube and ovary; very extensive adhesion of ovary to

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bowel and broad ligament; fimbriated extremity of tube bound to ovary. Separated from bowels, etc., raised the cyst, ovary, and tube, and tied off close to uterus and deep down in broad ligament. In trying to separate left ovary and tube, broke into a pus sac which opened into the bowel in the pelvis, and hemorrhage began very sharp. In order to stop this I tied as deep as I could in the broad ligament with a Staffordshire knot, passing the arms of the loop close to the horn of the uterus and outside the ovary. When this was drawn tight I found that it included the ovary and adherent fimbriated extremity of the Fallopian tubes to such an extent that I could not remove them and still have button enough to prevent the ligature slipping; therefore I cut out the free portion of the Fallopian tube, made a careful peritoneal toilet, put in a glass tube, and closed the incision. The patient made an uneventful recovery, and passed out of my immediate care.

Five months after the operation she wrote to me, and said she had menstruated twice without pain, the flow lasting about four days.

In 1890 she wrote to me to say that she had not menstruated for four months, had pain in the pelvis, a great deal of irritation of the bladder, nausea, vomiting, was getting stouter, particularly across the lower abdomen.

She came to see me, and on examination I found all the signs of pregnancy and could detect the fetal heart.

She returned to the country, and in due time gave birth to a healthy, well-formed female child.

Labor was tedious, about twenty-four hours, but child was delivered without instruments.

Here, then, Mr. President, we have eight cases of pregnancy reported by members of this Society alone, and all occurring within the past four years. It would appear, then, that, apart from the questions revolving about the mental and physical effects of the enforced menopause, the question of pregnancy had some standing. This becomes all the more apparent when we view the conditions under which it was accomplished in Cases 6, 7, and 8. Such cases are not only food for reflection, but a stimulus to further efforts in the direction of "conservation."

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THE CONSERVATIVE TREATMENT OF THE FEMALE PELVIC ORGANS.

BY WILLIAM GOODELL, M.D., LL.D.,

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ALTHOUGH a co-referee I have taken the liberty of changing the title of my paper to that of the Conservative treatment of the Female Pelvic Organs. This substitution of the more generic word *treatment* for the specific one of *surgery*, as will be seen, does not make me an enemy in the camp. But it enlarges the scope of the subject, and is more in keeping with my own views. For I hold that besides those strictly surgical there are other kinds of conservative treatment, which are especially adapted to the ailing organs of the female pelvis.

The question on which hinges the conservative treatment of the female pelvic organs is, What effect upon a woman has the removal of her appendages? Unquestionably there usually follow the annoyances of the change of life. These in my experience, are long spun out, because, when menstruation has been abruptly and artificially stopped, the change of life, especially in young women, takes a longer time to become fully established than when the menopause has been attained naturally. Consequently, months may elapse—nay, even years—before the victim of the operation escapes from the perspirations, the heat flashes, the skin tinglings, the numbness of the extremities, the nerve-stress, the nerve-storms, and all other vaso-motor disturbances which are so hard to bear.

Then, again, the unwelcome fact cannot be ignored that mental disturbances may be traced directly to the ablation of the ovaries. This is manifested by morbid broodings, by low spirits, by melancholy, by suicidal impulses and even by insanity. Every ovariotomist regretfully has met with such painful episodes in his practice. Glaevecke, who has made extensive researches on this subject goes so far as to say that "in almost all cases the mind becomes more or less affected, and not infrequently melancholia results." Keith* has stated that ten per cent. of his patients who recover from hysterectomy subsequently suffer from melancholia or from other forms of mental disease. Yet this result must have come in his cases, not so much from the removal of the womb, which is merely a muscular bag, as from the associated ablation of the ovaries, of which the womb physiologically is only the

* *New York Medical Journal*, July, 1889, p. 73.

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appendage. Sinkler, who carefully and impartially has studied this subject, believes that Glaevecke's "statement is undoubtedly too extreme, although the number of cases of insanity following oöphorectomy is large."*

Whether this deplorable event is due directly to the nerve-shock of the operation itself, together with its emotional environment; whether to the abrupt cessation of an habitual flow, or whether to the absolute need of the ovaries for mental equilibration, is yet an open question. We know, however, that sexuality is a potent factor in women, as well as in man, and that even certain sexual functions, such as coition, menstruation, gestation, parturition and lactation, of themselves tend not infrequently to disturb the mental poise. I am disposed, however, in a measure to attribute the attacks of insanity in those women who have lost their ovaries to their brooding over the thought that they are unsexed; and if brooding may be deemed in itself a mental aberration, Glaevecke's sweeping statement is not an extravagant one.

But, above all, the burning question is, does the removal of the uterine appendages affect the sexual sense of the woman or in any way unsex her? Here we have an embarrassing diversity of opinion. Some operators contend that in these respects castration does not change her at all; others, that it does so, and often very decidedly. The truth in such cases usually lies in the mean, as I shall try to show.

After a close study of this subject, it seems to me that the fairest comparison of a castrated woman is not to a castrated man, but to a woman who has reached the menopause before her time. Now, the natural change of life when fully established, but not until it is fully established, does very sensibly dull and deaden the sexual sense of women, which ultimately disappears in her long before virility is extinguished in man. Reveillé-Parise, in his excellent treatise on old age, enlarges on this fact and makes the period of gradually lessening sexual feeling in women to last from the age of forty-five to that of fifty, when this sense is usually lost.† This is most marked in those women who have reached the menopause much earlier than the usual time. Such women, it is well known, lose the sexual feeling very soon after the cessation of menstruation. Nor is the survival of the sexual feeling after the menopause so essential to the woman, because, when menstruation ceases, she loses the power of procreation, which, however, is retained to an advanced age by the man. Now, what happens in the natural change of life holds good in that artificially

* University Medical Magazine, December 1891, p. 176.

† Traité de la Vieillesse, Chap. ix.

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produced, with this important difference, that in the latter the sexual feeling is sooner dulled and sooner lost.

My own large experience would lead me to the conclusion that in the majority of women who have been castrated, the sexual impulse soon abates in intensity, much sooner than after the natural menopause, and that in many cases after the lapse of a few years, it wholly disappears. This tallies with Glaevecke's verdict, that "in most of the cases the sexual desire is notably lessened and in many cases extinguished." In corroboration of this statement I could cite many cases in point, but time forbids.

Dr. Gill Wylie mentions* as one result of castration, the atrophy, senile in character, of the external and internal genitalia, which makes coition often very painful. He refers also to a distressing hyperæsthesis of the vulva in young women who have been castrated. These conditions I have occasionally seen. One of my patients operated on two years ago who was unusually amorous before castration, now, not only has lost all sexual feeling, but she suffers much pain during sexual intercourse. Yet with true womanly devotion she has studiously kept her husband in ignorance of these facts. Another woman, operated upon not quite a year ago by one of my friends, "abhors sexual intercourse" to such a degree, on account of this hyperæsthesea, as to make her married life an unhappy one.

There are yet two other points of importance relating to the castration of women. The first one is, that sterility follows the complete extirpation of the uterine appendages. Now barrenness is a frequent cause of domestic unhappiness and is in itself often a great curse. Further, as woman advances in age, there comes an irresistible yearning for offspring, and she often repeats that despairing cry of Rachel: "Give me children or else I die." What physician is there of ripe years who has not been importuned by women, hitherto wilfully barren but now longing for children, to undo the mischief caused by preventive measures.

The next point is this one: The majority of physicians and laymen look upon women deprived of their ovaries as unsexed. Just as castration in the male, so castration in the female is deemed a sexual mutilation to which common consent attaches a stigma. No woman would marry a eunuch, and few men would wed a woman deprived of her ovaries. In my own practice I have known of several very sad cases of marriage engagements broken off, of marital infidelity, and of bitter estrangement between husband and wife, all of which could have been avoided had one ovary been spared, or, indeed, had a mere fragment of one been left behind.

* *American Journal of Obstetrics*; November 1891, p. 1374.

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To sum up then the foregoing statements: It is manifest that during the period of woman's menstrual life, her mental, physical and social welfare depend greatly upon the continuance of the catamenial and reproductive functions. Therefore the conservation of those organs which preside over these functions, is of the utmost importance, and should be so regarded by the physician.

As a help to the conservative treatment of the female pelvic organs, let me consider a few points. In the first place, the fatality of chronic disease of the appendages is greatly overrated and that factor need not greatly disturb us, or hurry us on to hasty operative interference by abdominal section. Too many appendages have been needlessly removed and too many lives have been needlessly sacrificed. Far more women perish from the operation of removing the tubes and ovaries than from their diseases themselves. In this opinion I am confirmed by Reamy who states that in forty years he had "not seen five cases of death directly traceable to ruptured pus tubes."* After the patient has safely passed through the acute stage of the inflammatory attack, her life is in very little danger. Chronic disease of the appendages usually affect the well-being of the woman, but they ordinarily do not threaten her life in any other way than by the wear and tear of prolonged discomfort. They may shorten her days, but fatal attacks of peritonitis or deaths from chronic abscess or from the bursting of a pus-tube into the cavity of the peritoneum are extremely rare.

To restore the health of a woman whose appendages are diseased, or to relieve her sufferings, abdominal section is by no means always necessary. Of the advantages of Polk's method of curetting the womb and of packing its cavity, I have had ample demonstration. Let me refer to one case: An uncommonly handsome and finely formed woman came under my care for nerve-prostration heightened by menorrhagia and by a constant nagging pain in the left ovarian region and down the right thigh. The cervix was torn and the womb hypertrophied. Both ovaries were swollen and tender, especially the left one; while the left tube was distinctly enlarged, tortuous and fixed. I repaired the cervix, curetted the womb and gave my patient rest, massage, electricity and topical treatment but not drainage. She was in many respects greatly improved, but the menorrhagia still persisted and the ovarian pain stubbornly held its own. She was such a splendid specimen of female humanity that I shrank from mutilating her by the removal of the offending appendages. Dr. Polk had just published his valuable paper on uterine drainage, so I sent

* American Journal of Obstetrics, April 1894, p. 525.

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her to him. He confirmed my diagnosis, and by the use of the curette and uterine drainage restored her to complete health. Here was a case so stubborn that it would ultimately have driven even me to the castration of my patient had not conservative surgery cured her.

Many women with adherent tubes and ovaries, and for the matter of that, some even with pus in these organs, suffer either no inconvenience whatever or very little indeed from that condition *per se*. I know numbers of such women, some with fixed appendages, and some with pus in their tubes, who now never need medical advice, and who are to all intents and purposes well. There are, again, others who have pains and aches only at their monthly periods. Let, however, this health break down from grip, from influenza, from malaria, from over-work or from nerve-strain ; then symptoms may arise from their hitherto latent pelvic lesions. Yet in most of these cases, no surgical operation whatever is needed, for, if the woman can be restored to her former condition of health—that is to say, that which she enjoyed just before her final breakdown,—she will lose her local symptoms and become symptomatically well. On this matter I can speak positively, for many a patient has been sent to my private hospital to have her distinctly damaged tubes and ovaries removed, who has been restored to health without a resort to the knife. By the use of rest, massage and electricity, by a general building up of the whole system, and by topical treatment, these have been cured by me, cases with all the objective and subjective symptoms of ovarian or of tubal abscess. In some few of these cases this treatment was followed by conception, pregnancy and parturition. In at least two such cases in which I urged castration, fortunately refused, the patients subsequently conceived and gave birth to living children at full term. Nor is my own experience isolated. At the last meeting of the American Gynecological Society, Dr. Robert A. Murray* gave a clinical report of six cases of double pyosalpinx. He had treated them by Polk's method of uterine drainage, and they all afterwards conceived and bore healthy children. In the discussion which followed the reading of this paper, Dr. Geo. M. Edebohls stated that he "had known a number of cases of true pyosalpinx get well, the women afterwards becoming pregnant." Lusk, Chéron, and Dolèris have had an analogous experience. The latter goes so far as to make the too sweeping assertion that eight-tenths of cases of damaged tubes and ovaries can be cured without operation. This opinion somewhat modified was sustained by several physicians who took part in the discussion at the International Congress of Gynecologists, held two years ago at Brussels.

* Transactions of the American Gynecological Society, 1893, pp. 345 and 346.

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The possibility of a closed up oviduct regaining its bore is warmly disputed, yet it is a well known fact that solid uterine fibroids of several pounds weight have been known to spontaneously disappear through retrogressive metamorphosis. At a meeting of the Obstetrical Society of London held June 7, 1893, Doran and others referred to forty such cases, and I myself have met with three that are analogous. So also will firm and extensive adhesions vanish away. Why, then, may not the thin tubal barriers and septa of inflammatory origin also break down and melt away. On the other hand great disorganization of the ovaries is not incompatible with conception and pregnancy, for it appears that a very small amount of ovarian stroma goes a great way. Atlee reports two cases in which, one ovary having been removed, the other became so cystic as to need *repeated tappings*, yet each woman not only menstruated, but conceived and gave birth to a child.* In one of these cases, an ovary having been removed many years previously, the other one becoming cystic was tapped twice before conception, twice before delivery, seven times afterward and was then extirpated. A number of years ago I had a case of intraligamentary cyst of the right ovary. After its removal which was attended with much difficulty and with much loss of blood, the left tube and ovary were found vitally attached to the pelvic wall and to the surrounding viscera. In view of the shock under which my patient was laboring, and of the dangerous character of these adhesions, I did not dare to detach or remove this appendage. Yet, not many months later, this lady, who hitherto had been sterile conceived and bore a healthy child. Dr. B. F. Baer has had a still more remarkable experience. In his case the womb and both appendages being firmly bound down by adhesions, he "by great effort" released the womb, tore the left ovary and tube "piecemeal" from their position and ligated their "shreddy pedicle." "The right appendages were found in an almost similarly diseased condition, and, when they were dissected loose, they were also in shreds, the tube having been torn off about two inches from the uterus."† Dr. Baer urged their removal, but he was not permitted to do so by a physician present, who was the brother of the patient, and had promised his sister that only one appendage should be removed. Fifteen months later the woman gave birth to a child at full term. To cap the climax, Robertson‡ mentions a marvellous case in point. He extirpated both the damaged ovaries from one of his patients, yet she afterwards con-

* Atlee, Ovarian Tumors, pp. 38 and 39.

† Annals of Gynæcology and Paediatry, January, 1894, p. 232.

‡ British Medical Journal, September 27, 1890, p. 722.

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ceived and gave birth to a child. His explanation is that he must have left unwittingly a scrap of healthy ovarian tissue in one of the stumps. This hypothesis is unquestionably correct so far as it goes, but it does not cover the insemination of the ovum and its descent into the womb, which can be explained only on the supposition that the lumen of one of the oviducts had reopened at the point where it had been sealed up by the adhesive inflammation set up by the ligature.

But, supposing simply therapeutic measures fail, or that the use of the curette and of uterine drainage are not followed by relief, and the surgeon is driven to surgical interference, must he, after breaking up the adhesions, always extirpate the now free uterine appendages? Many surgeons contend not only that the diseased appendage should be removed, but also that both appendages should be extirpated, even if one alone be damaged. This advice is given on the ground that the healthy one is liable in its turn to become affected. My own course under such circumstances would be, never to remove the healthy appendage unless the menopause had been already established; or unless there existed a good reason for hastening it on, such as the presence of a uterine fibriod or of uterine disease or the insanity of the patient making it undesirable for her to have offspring. On the other hand, should both ovaries be intrinsically diseased, and their tubes contain much pus, I should always remove both uterine appendages in their totality, no matter what the age of the patient might be. Generally, however, the pus is limited to the tubes, and in that case one ovary, barring its adhesions, which, of course, must be broken, is healthy enough to be left behind. In such a case, the tube alone, if possible, should be removed, and not the healthy ovary or the healthy ovaries—if both happen to be sound. Further, rather than wholly remove all ovarian stroma, I should try in such cases, unless contraindicated, to leave behind at least a small fragment. For in several of my cases, in which a piece of an ovary not larger than a bean was left behind, not any menstrual or sexual changes whatever took place in the women. Should the uterine appendages be merely adherent, and not intrinsically diseased to any extent I should, as a rule during active menstrual life, release them, and perhaps extirpate the worse of the two, but not both of them. Such methods of conservative surgery will be occasionally rewarded by the maintenance of fertility. Polk, Schroeder, Martin and others, have published cases in which after a mere fragment of an ovary had been spared, and indeed after resection of the oviducts, conception and parturition took place.

The aim of modern surgery is conservation. Limbs, members and

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organs are now saved, which, formerly would have been sacrificed. This is its glory, and this has been brought about by antisepsis. When, but a few years ago, gynecology was weaned from its obstetric nurses, it threw off its swaddling clothes and marched abreast of its sister sciences. But antisepsis by the glamor of success has so dazzled modern gynecology as to make it a spoiler rather than a conservator. Reform is here needed. This reform does not imply a return to the expectant therapeutics of our forefathers—"ancestral gynecology" as Chéron scornfully calls it. It signifies conservative gynecology—a golden mean between the let-alone methods of the old school, and the too radical and too hasty interference of the new school.

Dr. Lutaud, Paris.

I have been much interested in the papers. I lately had a case which interested me very much of a woman suffering with pyosalpinx supposed to be double. The woman, however, became pregnant. Two or three months later, she began to suffer and I decided to operate. The left tube was found free.

I agree with Dr. Polk that a woman enjoys better health when she has the function of menstruation and I approve of his method. I think it useful in endometritis and for examination of the womb. I have not seen a case where it has been used in hydrosalpinx. The observations of Dr. Polk show that even where only a small portion of the ovary remains pregnancy may take place. Even where a woman is unmarried the function of ovulation and menstruation is very important and I very much approve of the method of limiting the treatment to the diseased parts.

Dr. Matthew D. Mann, of Buffalo.

I came here to learn, not expecting to speak but I can not help saying a word. I am glad that these papers have been presented to the profession with the authority of the American Gynecological Society, because there is a prevalent idea that gynecologists remove all the ovaries that they can, often without good reason. A prominent member of one of our societies has asserted that gynecologists should be limited to the placing of tampons in the vagina, and should not be allowed to open the abdomen because they do unjustifiable operations. What has been said to-day shows that there is a decided reaction against such

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radical views as we are supposed to hold. The leaders of the profession do not hold these views and do not do these things, and these imputations upon gynecologists will be wiped away by the papers of Drs. Polk and Goodell.

I am entirely in harmony with the paper of Dr. Polk. I have been familiar with his methods for some time and have practiced them for a few years with gratifying results, although I have no cases as striking as those of Dr. Polk to report. I am quite sure that many of my patients are much happier and in better health and are more useful members of society, wives and mothers, on account of the fact that I practiced these conservative methods than they would have been had I taken out everything that seemed to be only slightly diseased or that I thought might become so later. I have frequently had patients express their gratification at the result. I will mention only two cases. A young girl about to be married was brought to me by her physician. I found a large mass on one side of the uterus. I opened the abdomen and found a hæmatoma of the ovary as large as an orange. There was nothing to be done but to remove it. The other ovary showed beginning disease. There were several sacs in the ovary filled with blood. Two-thirds of the organ were healthy. I excised the diseased portion and sewed up the ovary with fine catgut. She recovered perfectly and has remained perfectly well ever since, a period of two years. As she told me herself, had both ovaries been removed she would never have married. In a somewhat similar case an engagement was about to be broken off on account of serious disease of the ovary. I removed one hopelessly diseased tube and ovary and freed the other from extensive adhesions. She got perfectly well with a movable uterus, and was happily married a short time ago and is now pregnant. I consider these efforts against the indiscriminate removal of ovaries and tube and in favor of conservative abdominal surgery as very commendable.

Dr. Joseph Taber Johnson, of Washington.

A discussion in order to bring out both sides of a subject should be discussed on both sides and I hope to draw attention to some points of importance to the other side. It is well in considering a subject of this kind, to decide what conservatism means. The reader of the essay spoke of the division of our forces into two camps, the conservative and the radical. It is quite likely that those who have been relegated to the radical camp may not agree to be placed in that position. We are all conservative to a certain extent. We are all conservative in

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attempting the eradication of a disease which is destroying life and wrecking health, and the kind of operation which is radical in one sense is conservative in another. The radicals would be quite opposed to the position in which they are arbitrarily placed by the papers; in fact it is stated they believe in the total destruction of the menstrual molimem and motherhood, by the removing of the menstruating organs for the class of diseases to which these papers have referred. I do not know of any operator who cuts out the ovaries for the cure of salpingitis or cuts out the tube for slight inflammation where the abdominal end is open. Where we make a mistake in diagnosis and open the abdomen and find very little disease I do not believe there is a member of this Society who would cut out organs which were not so diseased that their removal would be recommended by the readers of these papers. Most of the cases operated on by experienced gynecologists are sterile anyhow. They have been rendered so by the disease. It is the disease which has put the woman in this condition and the operator relieves her of her trouble and saves her life, but *he* does not make her sterile.

The dangers of this partial method of operating are apparent. It is quite likely that in the hands of operators not so skillful as the readers of the papers these conservative measures would in a good many instances be followed by such inflammatory conditions that the last state of the patient would be worse than the first. It is probable that these inflammatory conditions would close the tubes they had left open.

I endorse everything Dr. Polk has said until he reaches the treatment of inflammations of the ovary and tube. I fear some of his operations have not been complete or successful in curing his patients, and that as a result of his so-called conservative work, second operations may become necessary to radically cure the trusting women whom he may have only partially relieved.

Another result of this discussion may be to return the laity to the condition in which it was years ago. The people have been slowly educated up to the point of bringing these cases in time, but a result of this public discussion will be that some cases, which otherwise would have been brought to the surgeon early, will, under a mistaken idea that conservative treatment will save their lives, not be brought until it is too late and many lives may consequently be lost.

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DR. HOWARD A. KELLY, of Baltimore.

I am glad of the opportunity to put myself on record. I think that in this connection conservation is the better word. I shall speak of conservation of the pelvic structures. Not many here will recall the beginning of this discussion. It was ten years ago. The question then was whether or not the other ovary should be left when an ovarian tumor was removed? Sir Spencer Wells had left one ovary in some two hundred of his thousand cases. To these women some two hundred and fifty children had been born. The disadvantage was the recurrence of disease in thirteen cases requiring a second operation with two deaths. We had then two deaths to two hundred and fifty children born. Now we are discussing the subject from a different standpoint, which is the advantage of leaving parts of structures which are in other parts diseased. I can best define my position in this matter by speaking of an experience of at least fifty cases. I shall refer to groups of cases.

Where I found a cyst of the ovary as large as a lemon a few years ago, I invariably removed the ovary, now I often rupture the cyst through the vagina. If the abdomen is opened I incise the cyst. In other cases where I remove a diseased tube, I will puncture cysts in the ovary, without thinking of its removal. In other cases, I will break up adhesions through the rectum although this operation has fallen into disfavor. If I open the abdomen where there are moderate adhesions I break them up and leave the structure. Dr. Day practiced conservatism in a case where I removed an ovarian abscess as large as my fist with the tube glued down, but left the other ovary. The dictum has been that in ovarian abscess the appendages on both sides should be removed.

The operation of bringing the uterus forward and attaching it is a conservative operation. These are cases of pelvic pain where in years gone by we had no hesitation in removing the structures.

I think as regards extreme inflammatory conditions of the appendages this work is in the experimental stage. I have had to make a second operation in these cases. I do not think that we can sufficiently diagnose the exact condition of the ovary and tubes in many of these cases and decide whether it is safest to leave or remove it.

Dr. Polk has used the term ovaritis. I do not believe that such a disease exists in the form of a chronic inflammation of the organ. I believe in leaving sound structure. Where there is a serious grade of inflammation, we shall often have to remove the organs.

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Dr. Florian Krugg, of New York.

It has been asserted that there are two camps, the radicals and the conservatives. If that is a fact, I belong to both. On Tuesday, I read a paper in which I held that where the appendages were diseased to such an extent as to demand their ablation *in toto* that it was a most wise and conservative process to excise the uterus at the same time; taking the ground that under these circumstances the uterus was useless but by no means harmless. I was criticised for removing a healthy organ simply because the woman was castrated. My reply was that I had never removed the appendages where the uterus was not diseased.

I am in accord with almost every statement of Dr. Polk. I might add my testimony to the cases that Dr. Polk has related as to the permanency of the cure. Within the last few weeks I have heard of the seventh case of pregnancy occurring in my own practice after coeliotomy had been performed and a part of one or both adnexa had been left and only the diseased portion removed.

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ETHMOIDAL DISEASE.

BY F. H. BOSWORTH, M.D.,

Professor of Diseases of the Throat in the Bellevue Hospital Medical College.

ETHMOIDAL disease, both from the anatomical character of the regions involved, and with reference to the symptoms to which it gives rise, differs essentially from diseased conditions found in any of the other accessory cavities of the nose. For whereas the maxillary, frontal and sphenoidal sinuses present to us a single cavity confined by bony walls, the ethmoidal cells consist of a large quadrangular mass of small cells, or trabeculæ, varying in size, and each cell more or less completely separated from its neighbor by a thin, bony partition. This is fairly well shown in Fig. 1, the turbinated bone having been removed. Hence while the problem which presents

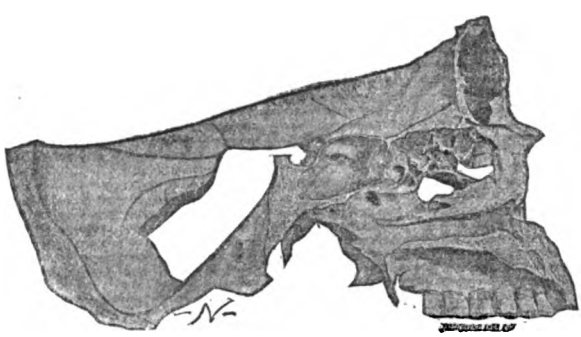

FIG. 1. The trabecular character of the Ethmoid cells shown after the removal of the middle turbinated bone. From a specimen in the writer's collection.

itself to us in dealing with morbid conditions in the other cavities consists simply in making an opening for proper drainage and irrigation, in dealing with diseased processes in the ethmoidal cells the radical arrest or cure of the disease, especially where suppuration has taken place, demands the opening of each of the large number of small cells which compose this mass. This is impossible. Hence we are compelled to resort to the breaking down of these small partitions within the ethmoid body, in such a way as to convert it into one single cavity. And here, perhaps, it may be proper to say, that in a diseased condition of any of the accessory cavities I think we will all recognize the fact that the tendency in every case of a simple inflammatory process which does not undergo resolution and which develops

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into a chronic inflammation, is to result in suppurative action, and thus the establishment of a more or less permanent pus-discharge through the normal opening, namely, into the nasal cavity. The other respect in which disease of the ethmoidal cells differs from that of the larger sinuses is in that whereas in the latter we have simply a purulent discharge, in the former, diseased action sets up a train of symptoms more or less neurotic in character, such as watery discharge from the nose; violent sneezing attacks; asthma; headache; neuralgias; which according to Berger and Tyrman* are usually intermittent in character; certain disturbances in the muscular control of the eyeball; asthenopia; and especially what has been called aprosexia. This latter really constitutes one of the most distressing symptoms of the disease, and has been described to me by patients as a sort of blanket over their brain, which interfered with mental activity and the free use of their faculties. It is what the Germans call *Retentions-Erschopfung*, and is defined in Billings' dictionary as "inability to fix the mind upon a subject or to retain a lesson, accompanied by headache, and due to over-study or prolonged irritation in throat or nasal passages."

The topic which I am asked to discuss to-day, as I understand it, is the surgical treatment of ethmoidal disease. Practically I suppose when we speak of ethmoidal disease we refer to suppuration in these cavities. The subject of ethmoid-disease is to me exceedingly interesting, and is I think perhaps the most interesting which is engaging the attention of the laryngologists. I hope therefore that I may be allowed to refer somewhat briefly to the whole question of diseased processes here, prefacing this by the statement that I regard ethmoidal disease as not only by far the most frequent of all diseases of the accessory cavities, but as of very much more frequent occurrence than we ordinarily have been taught to believe, as will be inferred by the statement that in the past five years ninety-eight such cases have come under observation in my private practice.

In the chapter on acute rhinitis, in all of our text books on throat diseases, Bosworth among others, there is described a disease characterized often by nasal stenosis, frontal headache, intraorbital pressure, asthenopia, watery discharge, and violent sneezing, which I very frankly confess I have rarely seen where I was enabled to detect a rational explanation of the symptoms in the morbid condition of the nasal mucous membrane alone, as seen by occular inspection. I contented myself with the old teaching that these symptoms were to an extent reflex in character. I do not hesitate to say that I believe a

* Dis. of ethmoidal and sphenoidal sinuses, Weisbaden, 1886. Compare also Schech.

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very large proportion of the cases of so-called acute rhinitis are really instances of acute ethmoiditis, and that such inflammation as may exist in the nasal mucous membrane is really secondary to the graver and more distressing conditions of the lining membrane of the ethmoidal cells. I thus endorse the statement made by that excellent observer Moritz Schmidt, of Frankfort,* who assigns these symptoms of a head-cold to an involvement of some of the accessory sinuses, not confining it however to the ethmoid cells.

Among the cases which I shall briefly analyze later are a number of instances, both of acute ethmoiditis and suppurative disease, which have had their onset in unmistakable attacks of La Grippe, which leads me to hazard the suggestion as to whether the influenzal type of La Grippe is not really an invasion of the ethmoid cells by the specific bacillus which is supposed to be the exciting cause of that disease.

In a paper on ethmoid-disease,† published two and a half years ago, I described five varieties of diseased conditions of these cells, which practically reduced themselves to three, viz : 1st, extra-cellular myxomatous degeneration ; 2d, intra-cellular myxomatous degeneration ; and 3d, purulent ethmoiditis ; I really think that practically we have but one primary morbid condition in the ethmoid cells, which progressively and with more or less rapidity develops into these three. In other words, that these three conditions are successive stages of one and the same disease. An acute inflammatory process of the mucous membrane lining these cells very soon either results in resolution or a chronic morbid process. Owing to the peculiar anatomical character of this membrane, a chronic inflammation tends to develop a soft, jelly-like thickening of the tissue, which takes on what we may describe as a myxomatous character. Now, this may persist for a somewhat prolonged period of time, giving rise to distension of the cells, with its train of symptoms already alluded to, which are, watery or muco-purulent discharges, violent attacks of sneezing, headache, intraorbital pressure, aprosexia, etc., and if the constitutional habit be neurotic, hay-fever and asthma, these symptoms being simply exaggerated on the occurrence of more or less frequently-repeated attacks of acute inflammation, to which the patient is liable.

The further course of this disease I take it to be, in a certain small percentage of cases, the crowding out from the ethmoid-cells, through the normal opening, of this myxomatous tissue, which presents in the nasal cavity in the form of small polypi. Not that I believe the large

* Krankheiten der oberen luftwege, Berlin, 1894.

† New York Med. Jour., Nov. 7, 1891.

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proportion of cases of nasal polypi have their origin in the ethmoidal cells, for Zuckerkandl* has demonstrated conclusively that this is not the case. As the result of this inflammatory process within the cells, the thin walls become distended and we have a somewhat curious development by which the outer wall of the cells yields before the pressure, and we have the middle turbinated bone crowded outward, and gradually an extension of these cells into this body. See Fig. 2 and 3. And here for the first time there presents a condition by

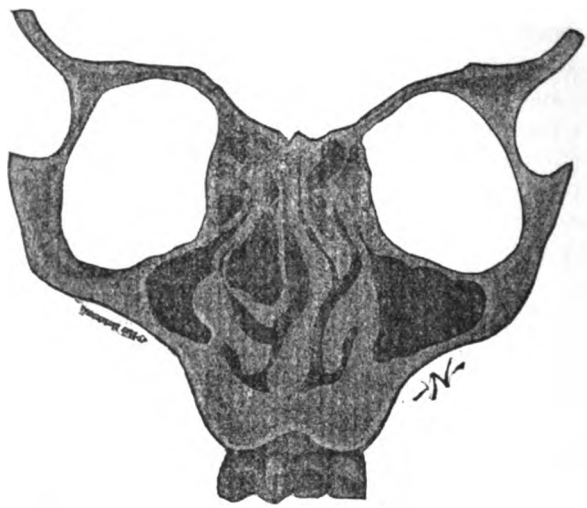

FIG. 2. Distension of the Ethmoid cells with extension into the middle turbinated body. (Zuckerkandl.)

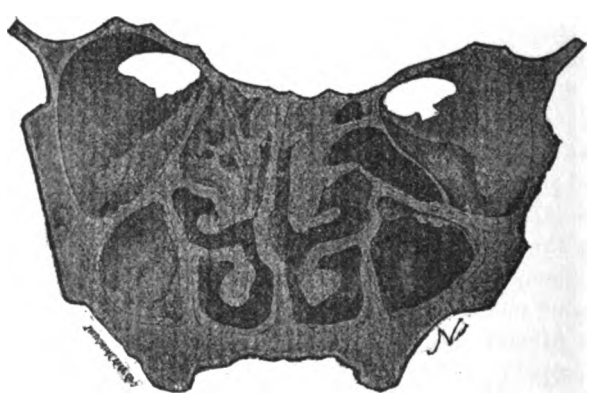

FIG. 3. Distension and extension of the Ethmoid cells displacing the middle turbinated bone. (Zuckerkandl.)

*Norm. und path. anat. der Nasenhohle, band 1, auflage 2, s. 360.

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which we may recognize a morbid process in the ethmoid-cells by ocular inspection through the nose; in other words, as far as rhinoscopic examination goes, in the inflammatory stage of the disease, this distension of the cells and extension into the middle turbinated body gives rise to a protuberance into the middle meatus, which is easily recognized, the middle turbinated body presenting as a rounded, ovoid mass, usually in contact with the septum and encroaching notably upon the middle meatus of the nose. At the same time this curious myxomatous degeneration of the mucous membrane and lining cells conveys itself to the mucous membrane covering the outer wall, which is now the middle turbinated body in the nose, and lends additional aid in the recognition of the condition.

These two conditions constitute what Zuckerkandl has termed "Ethmoiditis Superficialis" and "Ethmoiditis Profunda." The former term applying to the disease when it is confined within the cells and the latter designating the disease when the myxomatous degeneration has extended to the nasal cavity, making its appearance on the surface of the middle turbinated body.

The next stage in the development of the disease consists in suppuration. The time of its development may be very early or very late in the history of the disease, this being governed somewhat by adventitious circumstances. The method of its development seems very clear. The inflammatory process involving the membrane within the cells necessarily gives rise to hypersecretion, which materially contributes to the intracellular distension, and also results in a closure of the normal orifice. The necessary consequence is practically the formation of an acute abscess, which, failing resolution, soon develops into a chronic abscess, or chronic suppuration of the cells, for I believe it to be an almost universal rule that where we have a chronic inflammation of a mucous membrane with hypersecretion, in a closed cavity, the tendency of the process is very rapidly to develop into purulent action. We thus have established a chronic suppurative disease. The pus finds its exit through either the anterior or posterior ethmoidal cells; and I may state here, from a practical point of view, that the anatomical division of these cells into the anterior and posterior group, is of no special interest to us in dealing with the diseased conditions, in that I believe they are really converted into a single group of cells, by either a normal or a rapidly-established abnormal opening between the two. The pus makes its way into the nasal chambers through one of the normal openings, either in front into the hiatus semilunaris, or through the posterior opening into the superior meatus. Thus on ocular inspection we may find the pus making its appearance either

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from beneath the middle turbinated body or from between the middle turbinated body and the septum above. As a rule, the pus from the anterior and lower opening makes its way into the lower meatus and is expelled through the anterior nares, while the discharge from the posterior opening makes its way into the pharynx, giving rise to the symptoms so often complained of, viz: that of dropping in the throat, in which way the disease may be confused with an ordinary naso-pharyngeal catarrh; although I should say here, that the secretion of this latter disease is usually thick and adherent and expelled by somewhat violent nasal screatus, whereas a dropping in the throat should always suggest the great probability of an empyema of either the ethmoidal cells or the sphenoidal sinus.

While the most natural exit for the pus is into the nasal cavity, this is by no means its invariable course, as is shown by the large number of cases in which the pus escapes through the os planum into the orbital cavity, giving rise to exophthalmos and orbital disease. Furthermore we occasionally meet with exophthalmos from distension of the cells in the cavity, without escape of pus, as is still further and notably illustrated by the case reported by Bull,* of ethmoidal suppuration, in which an artificial puncture through the orbit was followed later by a spontaneous rupture into the nasal cavities, the ultimate cure resulting probably from the latter.

As the result of the persistent suppuration the lining membrane of these cells necessarily become soft and very much thinned. The much-discussed question of necrosis of bone never has interested me greatly, because I regard its importance as much overestimated. Zuckerkandl† states that he has never seen caries of the ethmoid-cells, thus contravening Woakes,‡ who describes what he calls a necrosing ethmoiditis as practically the sole cause of nasal polypi. The membrane covering the bone is so far thinned and softened that to the impact of the probe it gives the sensation of being exposed. That it is genuinely necrosed I do not believe. The odor moreover, which is almost constantly present in these cases in the later stage, is the odor of sulphuretted hydrogen developed from retained pus, and not the odor of bone-necrosis. Hence in making our diagnosis I should not be disposed to attach so much importance to the use of the probe as many writers do, depending mainly for the diagnosis on the appearance about the middle turbinated body of distension, and on the myxomatous degeneration of the mucous membrane covering it. This deceptive sense of necrosed

* New York Med. Jour., 1893, vol. lvii, p. 75.

† Loc. cit., p. 361.

‡ Nasal Polypus, Am. ed., Phila., 1887, p. 8, et seq.

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bone can be obtained by passing the probe either beneath the middle turbinated body towards the hiatus semilunaris or above it toward the opening of the posterior ethmoidal cells in the superior meatus. I have frequently detected it in a normal and healthy nose.

Our most important consideration of course has to do with the question of treatment, and that this should be instituted early in the history of the disease and before the suppurative process has ensued need not be urged, in view of the very serious discomfort and even danger which attends the stage of empyema, and the great difficulty with which it is brought under control after pus-formation has become chronic. In acute ethmoiditis the ordinary measures which our text books recommend in the treatment of acute rhinitis are indicated of course, and need not be dwelt upon, further than to say that of all measures I regard the use of the douche as most potent, directing that from one to two gallons of water rendered thoroughly saline be passed through the nasal cavities by means of the Thudichum douche, at least twice daily. The water should be as hot as can be borne. This instrument I regard as devoid of the danger to the ears, of which Roosa has warned us, provided that the patency of each nostril be tested before its use and that the stream be made to pass into the narrowest naris, thus emerging without obstruction from the most patent side.

When the disease has reached the chronic stage without suppuration, I believe that, other measures failing, surgical interference should be resorted to in all cases, as we here have to deal with an affection which does not tend to undergo resolution, but one in which there is imminent danger of suppuration setting in at any time. The test that this chronic stage has been reached is to be found in the swollen and distended condition of the ethmoid-cells, as shown by the projecting and swollen turbinated body, whether the mucous membrane covering it be in a state of myxomatous degeneration or simply turgescent. The object to be accomplished is to relieve intracellular pressure, and this is accomplished by uncapping, as it were, the ethmoid-cells. The steel wire loop of the Jarvis, or Bosworth snare is easily slipped over the projecting turbinated body, and the whole mass removed, presenting usually in the form of an elongated ovoid shell. This also reveals to us the condition of the mucous membrane within the ethmoid-cells, which may be either in a simple state of turgescence, or, as has not infrequently happened in my own experience, a soft, gelatinous mass of myxomatous tissue is found filling the cavity thus opened. If this does not drop out of place, it is very easily removed by the mouse-tooth forceps or the snare.

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After pus-formation has occurred the problem before us, as before stated, consists in converting the large number of small cells which compose these bodies into a single cavity, and establishing thorough drainage. In these cases the primary procedure is as before to uncap the cells by use of the snare, and after that I believe our best instrument consists in the use of the dental burr. Schaeffer, Moure, Cozzoline, Gleitsmann and others prefer the use of the curette. I have never been able to freely manipulate this instrument in the ethmoidal cells or to break down the trabecular walls by its use. Its manipulation is hampered, and its action, to my mind, seems inefficient and not radical enough in extent. Grünwald's sharp spoon is a more efficient instrument, but still hampered in its action. The same criticism applies to the forceps and sharp spoon as advocated by Zarinko.

That this disease can be radically cured, or even modified or controlled to any great extent, by the use of disinfecting lotions, as recommended by Lichwitz,* Hartmann,† Michel,‡ Schalle,§ Myles,‖ and Moure,¶ I do not believe. The small oval or round burr attached to the dental engine, or, better still, the DeVilbiss engine, in my hands has answered a better purpose than any other device. Manipulated with the DeVilbiss engine it is made to enter the ethmoidal cells either before or after they have been uncapped, when the trabecular-walls are easily broken down or burred away. Its motion can be instantly arrested at will, when it can be made to act as a probe, exploring the cavity for exposed bone or such parts as it is desired to remove. In this manner our operation becomes not only intelligent but, I think, absolutely safe, a consideration which of course is to be very carefully borne in mind, when we remember that we are separated in our operation not only from the orbit of the eye by an exceedingly thin plane of bone, but if we progress very far we are getting in close proximity to the base of the brain. I do not mean to intimate that at a single session the whole of the trabeculæ which constitute the ethmoidal cells can be opened out and thoroughly drained. In many of my cases the operations have been repeated a number of times at intervals varying from one week to a month, or even longer. During the intervals of course the patient is directed to use disinfecting lotions with as much intelligence and thoroughness as can be accomplished by the ordinary devices which we place in the hands of our patients for use.

* Dis. of Accessory Cavities of Nose, Bul. Med., Paris, 1893, lxxxv, 963.

† Quoted by Berger and Tyrman. Dis. of Ethmoidal and Sphenoidal Sinuses, Wiesbaden, 1886, p. 12.

‡ Idem. § Idem.

‖ Medical Record, 1892, vol. xlii, p. 607.

¶ Manuel pratique des Fosses Nasales, Paris, 1893, 2d ed., p. 535.

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I have no special suggestion to make as to the character of these lotions, as thoroughness of application is of more value than the special character of the antiseptic used. Moreover, it should be stated that the operation is not attended with anything like the pain that we should suppose when we consider the exceedingly sensitive character of the parts operated upon. Of course the procedure is not an agreeable one, but it is not an especially painful one, provided that we can sufficiently reach the parts as to thoroughly saturate them with a cocaine solution.

I may seem to the general practitioner to be discussing and describing not only radical but perhaps rough and even dangerous methods, but when we consider not only the great discomfort and distress to which a patient suffering from ethmoidal disease is subjected, but the still greater danger which threatens him, as has occurred, of involvement of other sinuses or neighboring cavities, such as the orbit, or even the development of the disease in the brain-tissue itself, I think there can be no question that these radical procedures are not only fully warrantable but absolutely indicated.

The charge has not been infrequently made in the past few years that intra-nasal surgery has been very greatly overdone. Some of our English friends have delicately suggested that we attack the nasal tissues "with the energy of the backwoodsman," "with more energy than discretion," with recklessness and without justification.

These charges are not deserved and are unjust. We do not confine our work to the saw and burr and drill. The very large majority of our cases are dealt with without resort to surgical measure, but we recognize a large number of cases in which other measures are inadequate, and then we attack, not with the energy of the backwoodsman, but with the trained and delicate hand of the surgeon, who recognizes unmistakable indications and carries them out, the gratifying result fully justifying the means.

As before stated, during the past five years I have recorded in my private practice ninety-seven cases of ethmoidal disease a brief summary of which I append. Of these ninety-seven cases three were carcinoma and one sarcoma of the ethmoidal cells, which may be left out of consideration. As regards age, three occurred in the second decade of life, fourteen in the third, twenty-three in the fourth, twenty-eight in the fifth, eighteen in the sixth, and seven in the seventh decade. Sixty-one were males and thirty-two were females. Fifteen cases were inflammatory in character without suppuration or polypoid degeneration. Of these, nine were cured, three were improved, and three disappeared. Twenty-nine cases showed myxomatous degeneration without

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suppuration. Of these twelve were cured, ten improved, and seven were seen but once or twice and their progress not known. Twenty-two cases showed myxomatous degeneration or fully developed polypi together with pus-discharge. Of these, nine were cured, ten improved, and three disappeared. Of the purely suppurative cases there were twenty-seven, of which eight were cured, twelve improved, both as regards subjective symptoms and the amount of discharge, while seven were seen but once or twice and not further noted. In nearly all these cases radical operative measures were instituted, as carrying out the suggestions already made. In many of them the simple irrigating measures failed to give relief, either to subjective or objective symptoms, and the cells were opened either by the saw or drill. In many cases both were used.

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SURGERY OF THE MAXILLARY SINUS.

BY J. H. BRYAN, M. D.,

Washington, D. C.

In considering the subject of the surgery of the maxillary sinus, it will be necessary to mention the various pathological conditions that affect this cavity. Among them may be mentioned inflammation of the lining membrane, varying in intensity from a simple catarrh, with its resulting accumulation of mucus, a condition known as mucocele, to a suppurative inflammation which may be either acute or chronic; foreign bodies, including insects; and morbid growths, which may be either benign or malignant. As the indications for treatment of mucocele and empyema of the maxillary sinus are the same, it will be considered as referring to the latter affection.

Of the surgical affections of the antrum, empyema, as far as frequency of occurrence is concerned, plays the most important part. While the study of suppurations of this cavity has made great strides within the last ten years, commensurate only with the advances that have been made in the whole field of rhinology, there still remains a great deal to be explained both in regard to the etiology and the resistance which many of these abscesses show to the most careful and persistent treatment. This disease, as we understand it to-day, is an entirely different one from that described by the older authors, who considered it an affection of rare occurrence, and described it as producing a marked bulging and thinning of the walls of the sinus. We, on the contrary, know that it is of frequent occurrence, and marked distensions of the walls of the sinus are rarely met with. The only conclusion to be drawn from their works is that they described, under the head of empyema of the antrum, cysts and other tumors of this cavity.

Without encroaching too much upon the subject for discussion, it will be necessary to make a slight reference to the etiology in order to appreciate the indications for treatment, for upon the cause of the abscess frequently depends our choice of operation.

Situated, as this cavity is, with the lateral wall of the nose for one of its boundaries, and with the roots of the 1st and 2d molar teeth in such close relation with the floor, it is especially prone to be affected

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by the extension of any pathological process that may exist in either one or the other of these localities. A wide difference of opinion exists as to which is the most common.

The treatment of abscess of the antrum has of late given rise to a great divergence of opinion as to the best method of opening the cavity, almost as much so as to the etiology, the development of rhinology causing a tendency on the part of modern surgeons to return to the practice of treating the sinus through the nose. The method first proposed by Hunter and put into practice by Jourdain of washing the cavity out through the natural opening in the middle meatus has found strong advocates in Störk, Hartmann, and others. In recent abscesses I have relieved several cases in this manner, but in chronic inflammations the procedure is too tedious and the results too uncertain to waste much time with it. In the majority of cases it will be necessary to make an artificial opening.

Among the earliest of the recorded operations, and one which it is only necessary to mention for the sake of historical interest, is that of Molinetti, who, in 1675, opened the maxillary sinus by making a crucial incision in the cheek and then perforating through the canine fossa. Naturally this operation found few advocates.

Up to 1886 there were three methods of opening the maxillary sinus in use, and all of these derived from the surgeons of the latter part of the 18th century. It was about this time that Cooper opened the antrum by extracting a tooth and breaking down the thin lamella of bone between the alveolus and the cavity above. This is, without doubt, the most readily performed of all the operations proposed. If there is a carious tooth present, which in the majority of cases, according to my experience, is the most common cause of the abscess, it should be extracted. Frequently the cavity will be opened by this procedure; if not, then it will be necessary to establish a communication with it by means of a strong trocar, or, which is much safer and more satisfactory, by means of a small trephine propelled either by a surgical engine or an electrical motor. This instrument should be directed slightly forward and inward, in order to penetrate the cavity at its most dependent part, and upon the withdrawal of the instrument it will be followed by a discharge of pus into the mouth. The opening should be sufficiently large to permit of the passage of a small metallic drainage tube, varying from 3 to 5 millimeters in diameter, after which the local treatment can be carried out. The advantages of this operation are: 1st, In many cases it removes the origin of the disease; 2d, That the opening is at the most dependent part of the cavity, and, therefore, the drainage is most perfect; 3d, The

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patient can readily carry out, to a certain extent, the necessary local treatment, which is a very great advantage when it becomes necessary to wash out the cavity several times a day.

There are, however, many opponents to this operation who argue, 1st, That it is frequently necessary to extract a healthy tooth; 2d, That the constant flow of pus into the mouth is very annoying to the patient; and, 3d, That by creating an opening between the mouth and the antrum, particles of food and pathogenic organisms may pass into the cavity and keep up the suppuration. The second and third objections can readily be obviated by wearing in the metallic drainage tube a small conical rubber plug.

If for any reason it is not desirable to extract a tooth, or if the anterior wall of the antrum should show signs of weakening and bulge forward in the canine fossa, the sinus can be opened at this point, as first recommended by Desault, and latterly by Küster, who has modified the original operation by making a mucoperiosteal flap and then perforating through the fossa, making an opening sufficiently large to admit of the thorough exploration of the interior of the cavity.

The operation of Bertrandi of perforating the hard palate, is one that is rarely resorted to now, and that it should be reserved for those old and neglected cases where the hard palate shows signs of weakening and the abscess is about to discharge into the mouth.

If for any reason it is desirable to enter the cavity through the nose, then the method proposed by Mickulicz, in 1886, or some modification of it, should be adopted. Mickulicz advised opening the sinus through the inferior meatus with a spear-shaped knife bent at an obtuse angle, and having a projecting flange to regulate the depth of the incision. This instrument is introduced into the nasal chamber with its point toward the floor, and when at a point just under the natural opening of the sinus into the nose, the instrument is turned outward, and, with a sharp thrust, the knife is made to penetrate the inferior meatus into the cavity. A large triangular opening is thus established sufficiently near the floor to permit of easy drainage and the ready application of antiseptic lotions. Owing to the troublesome hemorrhage that frequently follows the use of this instrument, it is better to make the opening into the sinus with the curved trocar and cannula of Krause, or with the antrum drill of Solis-Cohen. This operation is not practicable, 1st, When the nasal chambers are unusually narrow; 2d, When there is a deflected septum; and, 3d, When there is a marked hypertrophy of the inferior turbinated body. It has also the disadvantage in requiring all applications to be made by the surgeon.

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It will thus be seen that we have numerous procedures for opening the maxillary sinus, and while it is impossible to lay down any rule which would be applicable to all cases, yet in a general way it may be said that in recent abscesses of nasal origin, the intra-nasal operation will be found to offer many advantages over the others. In those cases due to dental caries, the alveolar method is the one indicated. The opening through the canine fossa should be reserved for those cases, be they of intra-nasal or dental origin, which have resisted treatment by either the method of Mickulicz or Cooper, where it becomes necessary to resort to more radical measures, as will be seen when we come to consider the local treatment it is frequently necessary to do.

I know of no affection that will try the patience of both doctor and patient more, and the final issue of which is so uncertain, as a chronic abscess of the maxillary sinus. The local treatment is, therefore, very important, and upon the careful and methodical manner in which this is conducted depends the successful issue of the case. After opening the cavity, it should be washed out with a mild antiseptic lotion. If the case be one of mucocele, or acute abscess, the inflammation will frequently quickly subside, requiring, in many instances, only a few irrigations. In chronic abscess, however, we can hope for no such speedy subsidence of the inflammation.

The local treatment may be said to consist in the frequent irrigations with antiseptic solutions; the dry method, first advocated by Friedlander, of insufflating iodoform, iodol, or aristol; the application of caustics; tamponing the cavity with iodoform or bichloride gauze; and, finally, when the suppuration continues in spite of everything that has been done, of curetting the lining membrane of the sinus.

Irrigation may be done by means of a strong syringe with a curved tip, or through an Eustachian catheter, filling the cavity to its fullest capacity, for it must be borne in mind that it is irregular in shape and it is frequently subdivided by bony partitions thrown out from the floor into smaller incomplete cavities. Unless the sinus is filled with the antiseptic solution, the source of the suppuration, especially if it be a localized one, may escape the applications.

I will not take up your time by enumerating the various antiseptics that have been used and are recommended in this affection, but will only state that so far as my experience goes, I have found the simpler and less irritating the application, the more rapidly the secretion of pus subsides. After the cavity is opened it should be washed out with a warm saturated solution of boric acid, and continued until all reaction has subsided, then a solution of the peroxid of hydrogen is

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injected, varying from one-half to full strength, according to the indications. This form of treatment is continued until all signs of suppuration have ceased, after which it will frequently be found advisable to use a mild astringent application before the flow of mucus ceases, and for this purpose I have found a solution of the sulphocarbolate of zinc, varying in strength from two to five grains to the ounce, advantageous. A great many cases will be met with that will not respond to this simple line of treatment. Then we must have recourse to one of the other methods already referred to.

As to the method of Friedlander of washing the cavity out once and then insufflating iodoform or some other antiseptic powder, I have not met with much success, principally owing to the irregular shape of the cavity it is impossible to deposit the powder on the whole of the inflamed surface.

In order to discover the underlying cause for the continued suppuration, it will be necessary to enlarge the alveolar or canine fossa opening sufficiently to admit the thorough exploration of the sinus with the probe and little finger. Elsewhere I have advocated the application of the principle of endoscopy to this sinus as an aid to its thorough exploration, and for this purpose, I have employed the endoscope of Dr. Otis with its tubes shortened, and to which has been added a small metallic mirror which can be introduced through the tube into the cavity, and the whole of its interior brought into view. This instrument, together with the probe and little finger, has revealed numerous causes for these prolonged suppurations, such as frequent accumulation of granulation tissue, ulcerations, extensive caries and necrosis of the walls of the cavity, foreign bodies, such as supernumerary teeth, cystic degeneration of the lining membrane, polypi, and other tumors which have not reached an advanced stage of development.

Ulcerations and isolated granulations located by means of the endoscope can be treated with suitable caustic applications on an ordinary applicator passed through the tube, thus doing away with the necessity of making a large opening for that purpose. If, however, as occasionally happens, the membrane is the seat of much granulation tissue, cystic degeneration, or polypoid formation, then the opening should be enlarged sufficiently to admit of a thorough removal of the diseased tissues by means of a sharp curette, the cavity then washed out with an antiseptic solution and packed with iodoform gauze. The operator in curetting the very vascular tissues from this cavity must be prepared to meet with a sharp, and sometimes a difficult, hemorrhage to control.

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Any foreign body, such as supernumerary teeth, or teeth pushed into the cavity through clumsy dental manipulation, necrosed or carious bone, or any bony plates that may have formed, must be removed through an opening sufficiently large for the purpose, and the sinus treated on general surgical principles.

The majority of cases of abscess of the maxillary sinus will respond to one or the other of the modes of treatment described above; occasionally, however, some cases, especially when associated with atrophic rhinitis, and where the lining membrane of the cavity has undergone atrophic degeneration, will resist all known methods of treatment.

TUMORS OF THE MAXILLARY SINUS.

Up to the year 1882, tumors of the maxillary sinus were supposed to be of not very frequent occurrence, but since the systematic study of the pathological anatomy of this cavity by Zuckerhandl, and more recently since the appearance of an instructive monograph on the benign growths of the antrum of Highmore by Heymann,* we know that they occur much more frequently than was formerly supposed. Heymann examined 250 heads, and in 500 antra he found 31 benign growths, that is about one in every sixteenth or seventeenth subject had a tumor of the antrum. In none were there any external evidences of their presence.

Tumors in this region are both benign and malignant, and they originate more frequently from the muco-periosteum than from the alveolar ridge. According to some authorities, about two-thirds of the morbid growths of this cavity are malignant.

According to the classification of Zuckerhandl, the benign growths of the antrum are divided into cysts, polypi, osteomata and fibromata.

Cysts of the antrum are of three varieties: those which are due to a dilatation of the follicles of the lining membrane, and they may be simple or multiple; cystic degeneration of polypi, and dentigerous cysts, of which there are the external and internal variety.

* Virchow's Archiv, Vol. cxxix, 1892.

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REMARKS ON THE SURGERY OF THE ACCESSORY SINUSES.

BY JOHN N. MACKENZIE, M.D.

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I have listened with great interest to the excellent papers of Dr. Bosworth and Dr. Bryan. Knowing that they were to precede me and fearful of the danger of too much unnecessary repetition, I have chosen simply to enter the discussion rather than prepare an elaborate article on the subject selected for debate.

Until within comparatively recent times the average general surgeon never examined the nasal chambers, except when in quest of a polypus or when his attention was irresistibly attracted to these cavities by the horrible stench of an ozœna. With many other common affections, such as, for example, deflection of the nasal septum, they had little or no concern. Even until our own time observers have busied their brains with the burning question whether the deflected septum is turned more frequently to the right than to the left, never concerning their faculties with the remedial aspect of the subject, and in the presence of that overshadowing conundrum the treatment of the condition has been lost. Even so radical a surgeon as Marcus Aurelius Severinus declared that inasmuch as the distorted system was doubtless placed in that position by the will of God, it would be eminently sacrilegious to interfere with such conspicuous manifestation of Divine Dispensation, and therefore it should be left severely alone. Not so with the antrum. Cowper and the two Meibomii, father and son, lent the weight of their influence to the necessity of investigation of antral diseases and to the surgical methods for their relief. In their day, more attention was paid to this subject than to many other more common surgical affections.

At a recent meeting of the British Medical Association, a distinguished surgical authority apparently went out of his way to affirm that scarcely anything had been accomplished in the domain of rhinology in recent years. While all who are not ignorant or prejudiced know that this is not true as far as the nasal passages are concerned, it must be confessed that the stricture trenches pretty closely upon accuracy in the case of the accessory sinuses. While we have made great

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advances in technique, while the dawn of a new era in the treatment of this class of affection is breaking, it must nevertheless be confessed that we follow very closely to-day the teachings of those masters of long ago.

The chief interest in the surgery of the accessory sinuses centres in the manner of reaching their interior and the consideration of this question naturally involves an enquiry into the most convenient and satisfactory avenues by which to approach them from without.

In regard to the antrum maxillare—the most generally interesting of the accessory cavities—the question arises, why not enter here through the aperture which nature has already provided—the naso-antral opening. This was the enquiry which apparently occupied the minds of the older surgeons until Cowper pointed out a better and easier method of procedure. When we remember that we sometimes have to search for the foramen in the fresh cadaver, we can readily appreciate the difficulty we may encounter in the living subject in reaching and evacuating the antrum by this method. Moreover, by incautious attempts at penetration injury may be done to the floor of the orbit with possibly disastrous results.

In cases in which the naso-antral aperture is not readily accessible, it has been proposed to remove the entire middle turbinated body or its anterior position. This, it seems to me, would only be justifiable as a last resort. When then, in addition to the inaccessibility of the naso-antral opening the discomfort to the patient, not only in the penetration of the sinus but also in the subsequent syringing is taken into consideration, I think we may lay it down as a good rule of practice, that this operation should only be undertaken when other methods are contraindicated or in cases in which from extensive loss from disease or congenital absence of portions of the intra-nasal framework, the aperture is readily accessible.

The operation of opening the antrum through the inferior meatus owes its present popularity in Germany to the advocacy of Mickulicz, who opens the antrum wall below the naso-antral opening and close to the floor of the inferior meatus. The same objection of inaccessibility of the point of puncture obtains here as in the case of operation through the naso-antral opening. The antral wall is often very dense at this point, causing difficulty of penetration and great pain, and the operation is not infrequently attended by severe hemorrhage. Should this operation be undertaken, the opening should be made with a drill or similar contrivance and not with the spear-shaped instruments used by German surgeons.

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In my judgment, by far the best method of entering the antrum is from the mouth, by extracting a molar tooth and drilling through the antral floor. Authorities differ as to which tooth should be taken. If there be a diseased tooth, sacrifice it by all means; but when a choice exists, I should extract the second molar.

The advantages of this method are sufficiently obvious. First and foremost, by it we secure the best possible drainage of the antral cavity—not only drainage through the alveolar opening but also through the naso-antral—drainage by counter openings, the greatest surgical advantage. In the second place, the operation is more easily performed than the nasal operation, the loss of blood is almost *nil*, and the patient himself can attend to the subsequent cleansing of the cavity—a process which may extend over months and even years —a consideration of no small value.

The objections which have been urged against this method are (1) that it frequently involves the sacrifice of a good tooth, (2) that there is danger from entrance of food into the antrum and (3) that septic infection may occur from the mouth.

In regard to the first objection there are few who would hesitate between the loss of a molar tooth even though sound, and the loss of the middle turbinated body or the establishment of a large opening in the outer nasal wall: whereas, if the tooth be diseased, by its extraction two birds are killed with one stone. The second objection is purely fanciful. The canula or other device used to keep the alveolar wound open may be suitably plugged, if necessary, and foreign substances thereby excluded from the antrum. But, as a matter of fact, food does not commonly enter the open antrum. To get there it would have to travel a long distance, for it should be remembered that the cavity is high above the lower margin of the teeth. I have known patients wear an open antral canula for years without the occurrence of such an accident.

As far as the third objection is concerned, infection is liable to occur through any open wound whether in mouth or nostril; but while the former is the lurking place of many a congregation of well-known and nondescript disease-producers, the latter can hold its own successfully against it as the abiding place of myriads of infective agencies. Which wound, too, can be kept cleaner and therefore safer from infection, that, easy of access in the mouth, or that in the depths of the nasal chambers?

There is one contraindication to the alveolar operation which I may mention, and that is extensive induration and thickening of the alveolar process and floor of the antrum. Should such a condition,

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which occurs in the edentulous, exist, the operation should be abandoned for one of the nasal methods; or the antrum may be entered somewhere above the sockets of the teeth. The latter method often gives good results, but the opening is necessarily small.

I have had very little personal experience with the drilling operations on the ethmoid bone, but I have had thrilling adventure with the almost fatal, and in one case actually fatal results of that procedure when undertaken by an unskillful hand. I do not wish here to be misunderstood. I believe that a large proportion of inveterate and rebellious nasal catarrhs are due to ethmoid disease, and can only be cured by surgical means addressed to the ethmoid bone. A great deal has been already accomplished in the surgery of this region and more good work will follow shortly. It is no portion of my creed that the ethmoid is only in exceptionally rare cases the seat of diseased action; but I must protest against the wholesale destruction of this bone of which we have in the past few years seen too many conspicuous examples. The ethmoid region is one to be explored only by the expert, and hence when I see around me inexperienced men, following the lead of too enthusiastic operators, drilling the ethmoid bone with a ruthless recklessness that almost causes coagulation of the blood, there looms up before my mind the well-known narrative of the lithotomist of Indian story, who, according to the legend, after he had operated successfully many thousand times for stone without a knowledge of the anatomy of the parts, threw down his knife and refused to operate again when the mysteries of myriad vein and artery in the track of his incision were revealed to his untutored mind.

Before considering the question of operating on the ethmoid, as for example in cases of nasal polypi with purulent discharge, it should be borne in mind that in the large majority of such cases the removal of the growth alone is sufficient to dissipate the concomitant discharge, and a reasonable time therefore should be allowed to elapse before going further with surgical treatment. Even if the discharge does not subside in a few days, much, if not all, may often be accomplished by simple topical means.

Much error has arisen, too, from the confusion of cause and effect in this class of case and much needless surgery has been done by those who accept the harmful doctrine, preached in recent years, that ethmoiditis, simple or necrosing, is responsible for the genesis of nasal polypi. That it may sometimes be the responsible agent admits of no doubt—as, for example, in the case of polypi found in syphilitics—but as far as my observation goes, a primary simple ethmoiditis (aside

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of course from that produced accidentally by traumatic causes) is very rarely met with and is more often due to the surgeon than to original disease. The myriad cellular chambers of the ethmoid and middle turbinated bone (which in reality is a part of the ethmoid) offer safe asylum for germs and become readily depots for septic infection. We should also remember that the brain is not far off, and therefore in this class of case as well as in all operations of the kind in the nasal and accessory cavities the operator should proceed with great care and under strictly antiseptic precautions.

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RELATION OF INFECTIOUS PROCESSES TO DISEASES OF THE NERVOUS SYSTEM: PATHOLOGY AND ETIOLOGY.

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THE subject with which we are concerned may be studied from either of two points of view; that of the bacteriologist, whose interest is rather with the nature and mode of action of the infective agent; or that of the neurologist, whose training has made him more familiar with the laws of reaction of the nervous system. It is with the latter aspects of the subject that this paper will mainly deal. When bacteriological doctrines and discoveries are utilized I shall try to confine myself within conservative opinions, an intention for which no apology is needed.

It is hardly necessary to dilate on the importance of the study of the etiology and pathology of the infectious processes as related to diseases of the nervous system. Every physician can find in his own notebooks abundant memories of the wreckage left by the epidemic of influenza which swept over us a few years since; and the literature of these affections is already large. Tetanus and rabies, if not common are at least formidable; and the cerebral diseases which follow scarlet fever, measles and tuberculosis are among their most dreaded consequences. Many of the nerve lesions of syphilis are so common as to be familiar, but there are many more which are more formidable because more obscure, and less easily combatted. It is not only, and not so much, the direct effects of infection which are most to be feared. The prostrating influence of the original struggle, though it may end in the apparent defeat of the infective agent, is liable to search the hidden weaknesses of the nervous system, and to lay it at the mercy of morbid agencies with which it would otherwise have been able to cope successfully. It is true that we should not exaggerate this tendency and that it is difficult to do it exact justice. It is a common thing to hear a person say that he has been in better health since an attack of typhoid fever, influenza and the like. But it is equally common to find chronic invalidism and the insidious onset of progressive degeneration referred to the same cause.

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The aggregate number of patients with affections of the nervous system which have come under our observation in the Out-Patient Department for diseases of the nervous system of the Massachusetts General Hospital during the last five years is a little over five thousand; and of these about five hundred had had, within a year before the onset of the illness for which they presented themselves, some infectious disease which might be suspected to have been at least a partial cause. About eighty per cent. of these affections were post-influenzal and post-syphilitic (even excluding the cases of paretic dementia and ataxic tabes with a syphilitic history, which numbered, respectively, twenty-nine, and seventy-one); the rest, in order of frequency, were cases preceded by diphtheria (fifty-three), typhoid (forty-seven), scarlatina (fourteen), malaria (thirteen), measles (nine), gonorrhoea (eight).

During the same period of five years, several cases of tetanus and of rabies were treated in the wards of the hospital, and I saw, in private consultation, three typical and fatal cases of rabies, all fatal; and two of tetanus, one of which proved fatal.

This list does not include the acute cerebral symptoms of febrile disease nor any of the affections which confine the patient to bed.

The various infective agents act on the nervous system to a small extent directly, but to a far larger extent indirectly; and an important part of the work of the physician in dealing with these diseases is to recognize these indirect influences that are at work. These are partly of nutritive character; such as the anæmia, œdemas, marantic thromboses, enfeebled heart-action, and digestive disorders, which attend or follow infectious processes. But besides these special causes of disease, which are so to speak external to the nervous system, there are causes of disease inherent in the constitution of the nervous system itself, to which the infectious processes, like so many other morbid influences, tend to give rein.

In this way the hereditary diatheses, which might never have been a practical source of danger, are enabled to make their way to the front; or degenerative processes like locomotor ataxia, either of syphilitic origin or not, which we may assume if we choose, to have been already set on foot but which were absolutely latent, rapidly gain head, so that they seem to have sprung suddenly into existence. Finally and most important of all, the relative weaknesses of the nervous system, of structural or quasi-physiological character, are searched out, and made manifest in the form of disease.

Of course, this danger is analogous to the two others but it is of a more subtle character.

The proclivities of the nervous system to disease, or what might be

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called its natural lines of fracture or cleavage, can be studied from various points of view. Dr. Hughlings Jackson has taken them up from the psychological side, in the presentation of what he calls the laws of "devolution" of the nervous system; Charcot, and his pupils, have studied them from the point of view of heredity and the mutual relation of the neuroses to each other and to other affections, such as the arthritic diseases. In similar manner, it may be shown that when the nervous system is exposed to the action of infectious poisons, there are certain ways in which it tends preëminently to break down.

When the nervous system is working in a perfectly normal manner, and not under strain it seems physiologically a unit. When the reverse is true it becomes evident that it is composed of many parts, of varying powers of resistance to disease.

The cardiac and vaso-motor arrangements work, perhaps, with exaggerated sensitiveness; and other diseases of quasi-physiological basis appear; while tracts and systems of feeble nutritive power begin to suffer. The psychoses and neuroses are the most prominent examples of this decadence. [See the discussion on "Typhoid-Spine," Osler (67).]

The problems before us are the following:

1. To inquire into the pathogenesis of the nervous affections which are obviously of infectious origin.

2. To investigate the claims of the series of affections which are only suspected to be of infectious origin.

3. To investigate the contributive causes which make the nervous system peculiarly liable to suffer from infectious influences.

The diseases with which we are principally concerned may be classified as follows:

RECOGNIZED CAUSES OF INFECTION.

	Organism known.	Organism unknown.	
Symptoms and lesions due not to the direct action of the specific organism, but to toxic products of bacterial action.	Tetanus.	Rabies.	Action of the infective agent confined mainly to the nervous system.
	Influenza? Diphtheria. (Gonorrhœa) Typhoid Cholera?	Syphilis. Exanthemata. Mumps. Whooping cough. Beri-beri.	Action of the infective agent not confined to the nervous system. Nervous lesions sometimes wholly due to the specific infection; sometimes only partially or indirectly.
Specific organism directly concerned in causing the lesions.	Typhoid. Tubercle. Lepra. Micrococcus. lanceolatus and the various other bacteria which excite meningitis and cerebral abscess. Malarial organisms. Actinomycosis.		

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NERVOUS AFFECTIONS

A. Probably due directly to specific infection; as indicated by the character or grouping of the symptoms.	B. Of which the relation to the Specific Affection is indirect or of uncertain character.
Tetanus.	Forms of neuritis associated with post-febrile disorders of septic states and metabolism.
Hydrophobia.	Effects of post-infectious cerebral anæmia, œdema, thrombosis, hæmorrhage.
Pertussis.	Disseminated sclerosis and other forms of spinal or cerebro-spinal scleroses.
Typical syphilitic affections.	Psychoses and neuroses of degenerative character.
Diphtheritic paralysis.	
Meningitis; cerebral abscess.	
Leprous neuritis.	
Beri-beri.	
Various forms of neuritis, myelitis and encephalitis.	
Actinomycosis of the brain.	

The data at our command for deciding as to these questions are partly experimental, partly clinical. It will be best to begin with the former.

A.1. It has been made pretty certain that the signs and symptoms that we think of as characteristic, in the nervous affections following infectious processes, are due far more to the toxic substances which are developed in the course of bacterial action than to the micro-organisms themselves.* The formation of tubercle and abscess, and the exudation of purulent meningitis imply the presence of micro-organisms, and bacteria are found now and then in the brain, spinal cord, and nerves, under various circumstances, but often without our being able to say as yet what their relation is to the lesions accompanying them.

It is only in the case of lepra that the bacteria may be said to actually invade the nerve cell or nerve fibre (Babes, and others), and even here the occurrence is not constant (Kuhne, Zambaco-Pacha). The presence of histological cell-changes after infection does not necessarily imply the direct action of bacteria, for they may occur in a typical form where only sterilized and filtered cultures have been used, as has been shown by Welch (93) for the case of diphtheria.

These toxic substances may circulate in a soluble form in the blood and be eliminated with the urine, and many researches have been made to study the toxic properties of these fluids with a view to

* See Editorial, Thera. Gazette, 1894, p. 185.

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seeing whether the original disease could be produced by inoculation with them. As yet, these attempts, some of which will be referred to later, have been only partially successful.

2. As regards the exact nature of the soluble toxic substances which are developed in the course of bacterial action, enough work has been done to show the extreme difficulty of arriving at conclusions, and to establish a few facts, of which the most important for our purposes are the following: These substances as they occur in the body, and even in artificial cultures, are highly complex, and contain elements of different chemical and physiological significance. [See, for example, Arnaud and Charron (6).] They vary somewhat according to the media or tissues in which the growth occurs, but the characteristic active principle has been obtained in some instances even where non-albuminous materials were employed for culture mediums (Ouchinsky). These experiments were done with cultures of diphtheria and the supposed cholera bacillus, and the products which were obtained gave the characteristic reactions of albuminoid substances. Brieger and Cohn (18), however, have succeeded in isolating the active principle of the tetanus poison in a non-albuminous form, which, though not absolutely pure, is of such power that the fatal dose for a man of 70 kilos, assuming his susceptibility to be relatively the same as that of a mouse, may be reckoned at 0.00023 grm. The smallest fatal dose of strychnia is about 100 times as large as this, and that of atropine about 150 times as large. It is not by any means certain that all the symptoms of poisoning are referable to the action of a single active principle, such as this, and the varying character of different epidemics of the same disease has suggested the idea that the poison itself is not always the same [see Gowers' (43a)].

It should also be remembered that under all circumstances poisonous substances are being formed in the body and are constantly eliminated by the kindeys, the bowels, the lungs, and the skin, and when this elimination is checked or arrested, as often happens in the course of infectious processes, an auto-intoxication takes place which bears its share in the production of symptoms [Bouchard (16)].

When we find ourselves in face with the problem just how we should explain the clinical and anatomical effects of infection, such researches as these should make us cautious. Bouchard (16), at the end of a series of researches as to the nature of the cholera infection, comes to the conclusion that there are two classes of influences at work; first, the poison primarily produced by the specific organism, whatever that may be; second, a secondary poisoning due to uraemia

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(itself a complex and variable cause), and that each of them have their own symptoms which may succeed or alternate with each other. How rarely, in dealing with the clinical results of infectious disease, do we take these secondary processes into account.

3. The virulence of the toxines produced by bacterial action varies according to the media on which the growth takes place, and although under ordinary conditions of disease it is rare to find colonies of micro-organisms growing within the central nervous system, yet it is interesting to note that Tedeschi (87) and Martinotti found that when cultures of anthrax bacillus were injected into the substance of the brain, the poisonous action was very much greater than when the injection was made elsewhere.

Acute local inflammations were also set up, and even animals usually not susceptible to the disease could readily be poisoned in this way. Furthermore, the cultures made from the foci of disease in the brain were found to show a multiplied virulence, and a poisonous substance showing special and characteristic properties was developed. Local inflammations may also occur from the injection of cultures of anthrax bacillus and of erysipelas into the spinal cord. (Tedeschi. (87) Bouges), though as regards the erysipelas it is doubtful if the experiment differs from the production of abscess by ordinary pyogenic organisms. I do not know of any case in human pathology where this intensification of virulence, arising from the use of the nervous system as a culture medium, is clinically observable, unless, possibly, some of the malignant cases of meningitis belong in this category.

Local inflammations of the brain and cord have been frequently reported, to be sure, as following one or another of the acute infectious diseases in man, but their origin is not wholly clear, and so far as is known they are not as a rule associated with the local colonization of the specific bacteria of the original disease. A number of cases of this sort have occurred as a result of influenza. Several of them were collected by Fürbringer (39), and others have been reported since then, some of which are referred to in a paper by the writer (75). Pfuhl (72) has recently described similar lesions as occurring after influenza, in which he found numerous bacilli which he regarded as identical with the influenza-bacillus.

It is not, however, admitted as certain that the influenza-bacillus is recognizable, and Pfeiffer (71) and Beck regard influenza as a purely local disease. At any rate, it seems certain that this local encephalitis is not necessarily attended with bacterial growth in loco morbi, and so far as clinical evidence is concerned the local inflammations fre-

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quently occur so late in the course of the infectious disease as to make it improbable that they are due to the specific organism itself. They occur also when no infectious disease has gone before (acute, primary, hæmorrhagic encephalitis Strümpell. See also Oppenheim (66); Lehrbuch der Nervenkrankheiten, 1894) and in connection with meningitis.

A change in virulence is not only determined by the character of the media on which the bacterial growth occurs but by the conditions of life of the previous generations of the organism in question. This is not the place in which to speak of this important chapter of bacteriological research. Especial interest, however, attaches to the modifications of the very important micrococcus lanceolatus, as described in the papers of Foà, Uffreduzzi, Kruse (53), Pansini, and Welch (93). It is still an unfathomed mystery why this organism, which appears to be present in the pharynx of a considerable proportion of normal individuals, should give rise of a sudden to a malignant epidemic of meningitis, such as that described recently by Flexner (37) and Barker. It is also interesting to note that although in man this organism probably penetrates the membranes, under favorable conditions, by way of the blood, as well as through the lymph channels, and although when brought experimentally in contact with the cerebral membranes of animals it grows and excites inflammation, yet it has not been possible to induce meningitis in animals by injections of the cultures under the skin.

According to writers above cited, the micrococcus lanceolatus sometimes appears as a special form of streptococcus, both in cultures and in the tissues. It may become a practically harmless organism and then again, under favorable conditions, regain its virulence. The histological changes to which the different forms give rise may also vary widely.

4. There has been much experimental evidence as to the part played by the nervous system in favoring the *invasion* of the body by the organisms of disease; and there can hardly be any doubt that when the tissues are imperfectly enervated, or when the vaso-motor system is incapable of prompt and effectual response to adequate stimuli, the power of resistance is greatly lessened, just as it is by cold, hunger, and fatigue. I do not consider it within the scope of this paper, however, to deal with the influences which favor invasion, but only with those which increase the liability to disease of the nervous system after the invasion has taken place. With regard to this point, experimental evidence is lacking, but the clinical evidence will be referred to later.

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5. The next point is in regard to the manner in which the bacteria and specific toxines act, and as to how far this action is really characteristic.

It would generally be admitted that this question is largely one of toxicology. Some of these poisons, if injected in sufficient quantity, may cause even death without giving rise to demonstrable lesion. The outposts of our knowledge in respect to the histology of the beginnings of disease are, however, being pushed steadily onward, and few persons can now be found who are still prepared to uphold the outworn idea that disease can exist without an anatomical or chemical substratum. If it is considered proper to call any given complex of symptoms a disease, then the anatomical change which gave rise to them should also be considered as a disease, and investigated as such.

Of course, it might be said of all the organs and tissues of the body, that there are only a limited number of changes by which they can respond to morbid influences, and the number of these possible changes is less than the number of influences that could excite them, so that, to some extent, the changes must repeat themselves, and that, therefore, it would be impossible to speak, in a strict sense, of any disease as, histologically, "specific." Frequently, however, the relative arrangement and distribution of the lesions, taken together, is characteristic even when the histological change is not so.

In *tetanus* poisoning, according to the researches of Brunner, the symptoms are due to an increase in the excitability of the spinal centers, analogous to that produced by strychnia poisoning, and for this we should hardly expect to find characteristic histological changes, though it is well-known that more or less gross changes are sometimes to be found in the spinal cords of tetanus patients.

It has been repeatedly observed that the experimental tetanus symptoms come on, not immediately after the inoculation of the poison, but at the end of an interval of latency. Such an interval ordinarily implies bacterial growth, but since in this case it occurs when the cultures have been subjected to a complex process of sterilization, Brunner refers it to the gradual summation of the influences which lead to the increased excitability of the spinal cord.

The most careful research into the character of the earliest lesions discoverable in cases where hitherto lesions have been supposed not to be found, are those described by Golgi as occurring in *rabies*. The nerve cell shows impairment of its nutrition by peculiar changes, affecting the size and contents of the nucleus. The cell-protoplasm, the endothelium of the vessels, and the neuroglia take part in the change, which is one first of irritation, then of reaction and repair.

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Golgi (41) denotes the whole process an "encephalo-myelitis parenchymatosa." The irritative stage is associated with active hyperaemia. He distinctly asserts that the histological peculiarities, taken by themselves, are not pathognomonic. They only mark where the poison has struck, and perhaps indicate the severity of the blow. It is especially noteworthy that most of these changes, as observed in rabbits, were found before the stage of complete paralysis had been reached, and similar lesions were in fact noted in the cerebellar cortex within four or five days after inoculation and before any symptoms had appeared. One is reminded by this statement of the peculiar form of parenchymatous neuritis observed by Gombault in guinea pigs, as a result of chronic lead poisoning, at a period when the animals appeared to be perfectly healthy. Golgi further notes that these changes which he found in rabies are very similar to those which he had discovered in chorea, in spite of the wide etiological and clinical difference between the two affections.

It will seem less remarkable that this infiltration of toxic substances should occur, even to the extent of causing the death of the nerve elements, without giving rise to demonstrable lesions, if we consider the case of ether. This is well known to be a nerve poison and if injected into the tissues in the neighborhood of a nerve—degeneration will follow. On the other hand, when an exposed frog's nerve is kept moistened at one point with ether, its power of conduction may be inhibited for at least forty minutes at a time (Bowditch), and yet on the removal of the ether the nerve may resume its functions, as if uninjured.

It would be interesting to compare the lesions which Golgi found with those described by Hodge (50) as characteristic of fatigue of nerve cells, as well as those described by Nissl and De Quervain as characteristic of the first stage of destructive metamorphosis of nerve cells from other causes.

So far as there is any characteristic of the changes produced by these complex organic poisons this probably consists in evidences of cell irritation such as have been described as occurring in diphtheritic poisoning [Welch (93), Babes (8)]. This tendency is positively characteristic, perhaps, for the endothelium of the vessels and the neuroglia, but less so for tissues like the nerve cells and fibres, which have certainly no marked tendency to subdivision and multiplication.

It may be noted as a bit of caution that minute changes such as Golgi (41) describes are only to be accepted as pathological when made by trained observers, and under the most favorable conditions of preparation. Even then, it might not always be easy to say that

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we were not dealing with the effects of fatigue or of œdema, and still more difficult to distinguish between the action of specific toxines and that of the toxic products of disordered nutrition. A good deal has been made of vacuolation of the nerve cell as pointing to some toxic influence, and Campbell (20) has devoted an interesting paper to this subject. Vacuolation has, however, been noted under such varying conditions that one may be allowed to wait for a more general consensus of opinion as to whether it indicates anything more than one of the ways in which nerve cells regularly degenerate. It certainly affords no means of distinguishing between bacterial poisons and others (20[a]).

When we pass beyond these minute parenchymatous lesions, the change which seems most characteristic is vascular congestion with infiltration of leucocytes, often associated with hemorrhage and perhaps due to endothelial changes, at least in part. This tendency has been so often discussed with relation to infectious processes in other parts of the body that it need not detain us here further than as suggesting that the œdema so produced may in itself be a cause of some of the associated nerve lesions. It might, for example, cause degeneration of nerve fibres, by pressure or disturbance of circulation, though it would not, so far as we know, cause such destruction of nerve cells as is seen in polio-myelitis. In general, when the hemorrhage and exudation are of infectious or toxic origin there is probably at the same time an actual infiltration of poison into the nerve elements and the other tissues in the immediate neighborhood of the affected vessels, but whether this infiltration precedes the congestion or is a necessary factor in its production and localization, is unknown.

More important than the histological changes described by Golgi is the distribution of the lesions which he noted. There would have been no special reason to suspect that the brain and cerebellum as well as the medulla and spinal cord would be involved in rabies, and the fact that this is found to be so, assuming that the observation is correct, is an additional reason for thinking that many poisons which seem to be very limited in their action, judging by the symptoms which they excite, are really universal nerve-poisons, and will excite widespread lesions if time enough is given them.

The experimental study of the nerve lesions of *diphtheria* has not been especially fruitful. Good observers have repeatedly failed to discover histological changes to account for paralyses experimentally induced, and although changes have been found in man, especially in

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the peripheral nerves,* yet they have been occasionally missed even by recent observers [Bikeles; Obersteiner's Arbeiten, Part II] and perhaps represent only a terminal stage in the process.

Quite recently an experimental research by Stscherbak (86) has given more positive results. The changes which he observed can be divided for convenience sake, into two sets; those involving the peripheral nerves, and consisting in well-marked degeneration which on the whole corresponded in position and intensity to the paralytic symptoms; and those involving the spinal cord, membranes, nerve-roots and the muscles. The latter he regarded as rather incidental than typical.

It should be said, however, that it is not the occurrence of neuritis which is characteristic of diphtheria, but the course and grouping of the toxic symptoms. These are, as a rule, so peculiar that we are justified in considering them as specific. The characteristic paralysis of the palate has been supposed to be due to a local spread of inflammation, but Gowers (43) is certainly right in saying that it may occur when the inoculation was in some other part of the body than the throat, or possibly, even when no primary local process of recognizable amount has gone before.

The next question to be considered, and one which is especially important to the neurologist, is with regard to the influences which increase the vulnerability of the nervous system in relation to the infectious processes,—the contributive causes of disease.

The question is really divisible into two parts, which need to be carefully distinguished. The first concerns the influences which lessen the power of resistance of the body as a whole to invasion by the organisms of disease, the second concerns only the influences which increase the vulnerability of the nervous system, or lessen its power of resistance, in respect to the poisons produced by the organisms after the invasion has taken place.

The distinction is an important one. A healthy animal, with intact mucous membranes, might resist the onset of infection, while even a healthy nervous system might not be able to withstand the action of a specific virus. Indeed, it is open to question whether the specific poisons do not affect healthy nervous centres more strongly, sometimes, than enfeebled nervous centres. For the response to the action of the poison, in the case of tetanus for example, though it takes the form of a symptom of disease, is often in reality a sort of reaction which implies a certain degree of previous vigor. It is obvious that a difference

* For an excellent account of the pathological anatomy of diphtheritic paralysis see Gowers's Diseases of the Nervous Syst. 2d Ed.

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would be likely to exist between the character and degree of the functional response to acute intoxication, and the liability on the part of the nervous system to suffer local lesions of nutrition which experiment has shown to attend toxine poisoning as well as the direct action of bacteria.

Finally, besides the influences which favor or impair the power or resistance of the body to the primary bacterial invasion, those influences should be considered which concern the degree to which the organisms of disease will be able to disseminate themselves, or, conversely, the degree to which a local reaction may be substituted for a general infection.

The whole great subject of the lessening the resistance of the tissues to bacterial invasion, induced by cold, starvation, and fatigue, and impaired innervation is of engrossing interest,* but can only be referred to here in so far as is necessary for sketching briefly the part played by the nervous system and its diseases.

The nervous system influences the resistance of the tissues to invasion; first, because it is necessary that the various tissues should be properly innervated in order that they may have that power to resist disease which constitutes health; second, because the vascular dilatation required for a protective local reaction cannot occur in a normal manner unless the vaso-motor system is capable of vigorous response to the adequate stimulus.

If the remarkable experiments of Bouchard (17), Charrin, Gamaleia, Buchner, and others justify the conclusions which have been drawn from them,—and so far as I know these are generally accepted, an extraordinary process sometimes takes place at the outset of an infection, in which the nervous system plays a prominent part. It had long been known that the inoculation of a sufficiently large amount of a virulent culture of various sorts was followed quickly by death, without local reaction. What is the reason for this result? The long series of experiments above referred to seem to have established that the failure of the local reaction is due to the paralysis of the vasodilator nerves by an early action of a diffusible element of the virus introduced with the culture.

It is impossible to enter here into further detail. Taking the whole course of the infection together, several different sorts of vaso-motor reactions are found to occur, with corresponding lesions and clinical results. (Arnaud (6) and Charrin.)

*(See a fine presentation of the subject by Charrin (17), in the Traité de Med. Tome I,)

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We may now inquire once more in what ways the diseases of the nervous system, and the influences tending to impair its vitality, affect the liability to the introduction and spread of the infection, in the different stages of the process as just noted.

It has been shown (see Charrin) that section of the nerve of a part renders the tissues more liable to suffer from infection, and it can hardly be doubted that the ulceration of the cornea after division of the 5th nerve is a case in point [see a recent address by Ferrier, on the Functions of the Cerebellum, etc., Lancet, 1894] but in most cases of nerve injury as met with clinically, secondary influences of non-infectious character rapidly intervene to complicate the situation.

The question of more importance for us is, what conditions favor the infection of the nervous system itself. And here come in the immensely important class of influences, not clearly defined, which we must call "predispositions," for lack of a better term.

Experimental evidence is not of much service here, for the attempt has scarcely been made to differentiate between healthy and diseased animals as regards the tendency of the nervous system to suffer from the effects of infection, *considered apart from the liability to invasion.* In human pathology it is certain that predispositions act as contributive causes to affect the liability to the indirect effects of infection, but less certain whether they increase the liability to the direct effects.

Clinical observation has made it clear that the nervous tissues of certain persons are prone to suffer, both in their nutrition and in their efficiency. The clinical history of diseases of the peripheral nerves gives a good example of this. Alcoholics are more likely to be affected with lead paralysis from a given exposure, than non-alcoholics. The nerves of many persons who are what is called healthy are in a very unstable state, and a slight amount of pressure or anæmia, or additional disturbance of nutrition from a new poison suffices to tip the scale. The tendency to infectious catarrhal affections which is so marked a feature of some forms of neurasthenia may rest in part on a direct impairment of innervation, but here also the conclusion is not positve, because passive congestion comes in as a complication. So also, it is difficult to prove that persons of what we ordinarily call a feeble nervous system are unusually prone to suffer from the direct effects of infectious disease, but they are very prone to suffer from the indirect effects.

B. Leaving now the experimental evidence and turning to the clinical facts at our disposal, we may say that the diseases of the nervous system with which we are concerned can be conveniently divided into two classes :

1. Those which follow well recognized forms of infection.

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2. Those which occur independently of recognized infectious processes, but which on other grounds are suspected of being of infectious origin.

As regards the first class, our problem is to discover the causes which bring about these post-infection disorders, separating so far as possible the direct from the indirect influences. The post-infection diseases which have the best right to be called specific on account of the definiteness of their clinical characteristics are tetanus, rabies, whooping-cough, diphtheria, and, in a sense, the direct nerve lesions of syphilis and tubercle. The diseases, on the other hand, which have the least right to be called specific are the post-infection psychoses and neuroses, acute focal encephalitis and myelitis, neuritis, the spinal scleroses. These affections occur as a result of such a variety of causes besides the infectious diseases that we must consider one important element in their production to be some natural weakness of the nervous system. They may be said to indicate what may be called the natural lines of fracture or cleavage through which the nervous system breaks, in response to general strains of various kinds.

Perhaps it may be shown for most of them that they do not occur except as a result of influences which may in a general sense be called toxic; but they are certainly not for the most part, pathogonomic of specific toxines.

If chorea, poliomyelitis, beri-beri, Landry's paralysis, herpes zoster, should prove to be exclusively of infective origin it would be fair to say that they also were to a considerable extent specific.

The arguments with regard to this point will be considered later.

In the first stage and also in the course of all the infective disorders, nervous symptoms mainly cerebral and of striking character are liable to occur. Little is known of them in a pathological sense. The temptation is to refer them directly to the toxines of the primary disease, but in the absence of positive information, it should not be overlooked that there are various influences present, which might give rise to them, and that an intimate knowledge of psychology is necessary for an understanding of their genesis. The functions of the brain are both complex and interdependent, and a relatively slight nutritive disorder, affecting the equilibrium of its different parts, may excite an attack of mania somewhat as it might excite a bad dream.

So, also, later in the course of these affections, paralysis of various sorts, of cerebral and spinal origin, are liable to occur. These are rather incidental than typical, and are due largely to œdema, thrombosis, embolism, hemorrhage (see, besides other papers, Bassette, Lob).

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With regard to *tetanus* and *rabies* and *pertussis*, there is nothing new of general pathological interest, beyond what has been already noted.

Neither would it be worth while to attempt, here, even a cursory survey of the typical *syphilitic* lesions of the nervous system, the subject is so vast and has been so much discussed.

As regards the less common lesions of syphilis, I will refer to two forms which seem to me of decided importance.

One of these is a form of myelitis closely resembling the ordinary transverse dorsal myelitis, but differing from it in some respects, and strongly dependent upon syphilitic infection. The disease is of slow development, and the spastic element is strongly marked in proportion to the paralysis and the loss of sensibility which are relatively slight. The patients may improve, especially under the influence of an energetic specific treatment, while, at the same time, neither the symptoms nor the improvement point to typical gummatous disease. This form was first descibed by Erb in the Neurologisches Centralblatt, No. 6, 1892, under the heading of Syphilitic Spinal Paralysis. Since then, communications confirming Erb's view have been published by Kowalewsky (Neurologisches Centralblatt, 1893, No. 12) and by Kuh (Deutsche Zeitschrift fur Nervenheilkunde, 1893, III, 6).

The second form of syphilitic disease of which I wish to speak is that where more or less typical lesions of syphilis are associated with those of tabes. This is a very important connection, because cases of this sort are perhaps those where the symptoms of tabes yield, in some measure, to anti-syphilitic treatment. Dinkler (Deutsche Zeitschrift fur Nervenheilkunde, 1893, 4 and 5) reports a striking case of this kind and gives references to the earlier literature.

As regards *diphtheria*, there is nothing of great importance to add to what has been said above, but the conflicting nature of the views respecting the character of the nerve lesion is well brought out by a comparison of the comparatively recent descriptions by Gowers, Arnheim, Hochhaus, and Cramer. Gowers and Arnheim describe the neuritis as parenchymatous, while Hochhaus and Cramer think that in diphtheria (as in the other infectious processes) the primary change is vascular and interstitial, and Hochhaus refers many of the symptoms to pressure of œdema. Gowers quotes an interesting observation by Boissarie (Gaz. Hebd., 1881, Nos. 20 and 21) describing a series of cases occurring in Paris of paralysis of the palate, eyes, limbs, heart, "perfectly like that which occurs after diphtheria, and accompanied by albuminaria. The remarkable fact is that in these cases of primary palsy there is no history of preceding sore throat, and in several of the cases distinct diphtheria followed the paralysis, which

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lessened during the throat affection. Some of these cases of primary palsy seem to arise by infection." At the same time there occurred a series of cases of severe diphtheria. This observation had special interest for me because it has seemed to me that several severe cases of polyneuritis without apparent cause, which I have seen during the epidemics of influenza, yet without primary catarrhal symptoms, might possibly still be due to the influenza poison. Oppenheim (Lehrbuch, 1894) also refers to the fact that the typical paralysis sometimes precedes the outbreak of local diphtheria. He quotes Eisenlohr as reporting cases of a toxic, but not diphtheritic form of multiple neuritis in which the palate and pharynx were paralyzed, and says that such paralysis has also been observed after erysipelas. These facts are of importance because they bear out the view which I have tried to maintain below that the nervous system lies open to attack much more in some directions than in others.*

With regard to the pathogenesis of the other group of affections, however, which follow infectious processes but can hardly be called specific, something more needs to be said. Although it is probably true that they do not often occur unless a toxic substance of some kind is circulating in the blood, yet there are reasons for suspecting that they would not even then occur unless the nervous system was prone to degenerate after that fashion in consequence of peculiarities of its construction, or mode of nutrition, or its functions.

There are two agencies at work which lend, each its share, in varying measure, to the production of the symptoms and lesions in these various diseases; the toxic cause on the one hand; the nervous system with its peculiarities of response on the other.

In the discussions held hitherto, too much importance has been attached to the infecting agent as explaining the characteristics of the post-infection diseases. I should like to reverse the picture, and point out, even with disproportionate emphasis, the importance of the part played by the peculiarities of the nervous system.

Among the post-infection disorders of the nervous system there is none so common as neuritis, involving usually many nerves in a symmetrical manner, but sometimes single nerves. There can be but little doubt that it is almost always toxic in origin, but it follows so many different causes, or partial causes, as to deprive any one of them of the right to be called specific, except perhaps where the grouping of the symptoms is characteristic, as in diphtheria.

From the point of view which interests us now, the different

* [Bikeles (loc. cit.) has recently published an observation showing that degenerative changes in the posterior columns of the spinal cord may follow diphtheria.]

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varieties of neuritis may be divided into those which follow known infectious processes, and those which occur independently. The interest which attaches to the latter group centres on the cases of acute, often hemorrhagic, primary neuritis, because these cases seem to indicate preëminently the action of some powerful toxic agent, of the nature of which, however, we know as yet practically nothing.

Finally, the question is still undecided whether the acute ascending paralysis of Landry belongs in the same category with polyneuritis or not.

According to the view of those who believe in this relationship and among them are such careful observers as Ross* and Leyden,† there is no marked difference in clinical history between selected cases of spreading neuritis, of the ascending type, and the affection to which the name of Landry is attached, and the mere absence of demonstrable lesions does not, they think, justify us in setting apart the latter cases as belonging to a special group.

On the other hand, there are not wanting thoughtful and conservative pathologists, among whom Hun‡ may be mentioned, as having given the subject a careful revision, who hold that so long as there does appear to be a class of cases where the disease is generally fatal and yet no definite lesions are found, there is nothing gained by lumping them, for the sake of pathological unity, with demonstrable cases of neuritis.

Both of these arguments have their value, and it is a gain to be able to recognize this fact, even though a positive conclusion is beyond our reach.

It is important to strive after pathological unity, and to learn to recognize the bond between conditions which are ostensibly unlike. It is equally important to realize that the reasoning which tempts us to substitute theory for observation is a dangerous weapon.

It seems to me that this much can be safely said: that the mere absence of demonstrable lesions in a given case of infectious disease cannot be taken as proof that the pathogenesis of the symptoms is not the same as in another case where lesions were found. [See a discussion in the Berliner Klin. Wochenschr., June 4, 1894, p. 546.]

The history of tetanus and diphtheria and perhaps rabies gives warrant for this assertion. For in some instances of these diseases, few or no lesions are discoverable, while in others, the lesions are more extensive than the symptoms would have suggested.

* A Treatise on Peripheral Neuritis, 1893.
† Z. schr. für Klin. Med., vol. xxiv.
‡ N. Y. Med. Jour., May 30, 1891.

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Again, we do not know with certainty the original cause either of the symptoms or the lesions of multiple neuritis in general.

It is impossible to disprove the view that, at least in the acute cases, the symptoms may be purely toxic, and the histological changes secondary to some central, trophic influence. I recall again, in this connection, the fact mentioned above with regard to ether. (Bowditch: Report of Physiological Society, 1894.) Although it is a tissue-poison for nerves, yet if applied directly to the exposed nerves of a frog it will cause temporary paralysis which may be kept up for more than half an hour and yet pass finally away, presumably without leaving lesions of great amount.

Part of the symptoms of acute hemorrhagic infectious neuritis, are, unquestionably of peripheral origin, those namely which are due to the pressure of the endoneural exudation. It is however far from certain whether death, in these cases, is due to the neuritis rather than to poisoning of the nerve centres.

Leyden, as well as various other observers, has called attention to the fact that in some of these cases changes in the spinal cord are discoverable, one of the commonest of which is swelling of axis cylinders.

This was very noticeable in a case reported by myself where death occurred at the end of a week, from respiratory paralysis. (N. Y. Jr. of Nervous and Mental Disease. 1887.)

As regards the more chronic forms of neuritis, which follow known infection, there can be but little doubt that they are toxic in origin, but it is for most of them quite uncertain whether the poison is identical with that which caused the primary infection, or is due to secondary disorders of metabolism.

The peripheral nerves (or their trophic centres) are singularly sensitive to poison of almost every kind. Ross enumerates forty causes of neuritis and a few more could be added to the list.

Among those recently noted are, puerperal conditions (Mœbius Bassette (11): erysipelas (Grasset); septicemia (Lloyd and Riesman); pyelonephritis (Dana); suppuration of the pleura (Fiessinger).

It is true that the lesion is not precisely the same in all these cases. In the form which follows acute infectious diseases, the morbid process may centre around the blood vessels, and is said to be, in general, rather interstitial than parenchymatous (Cramer (22)). The spinal cord is apt to be involved with the nerves in these cases. In some instances the parenchymatous degeneration may be due to the pressure of œdema. In the acute (hemorrhagic) form, which is

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supposed to be of primary infectious origin, the trunks of the nerves and the nerve-roots may be involved, as well as the peripheral portion. Different varieties of degenerative neuritis are also described, such as the "periaxillary" and "segmental" forms.* It may be that careful research will show that where an irritant organic poison is circulating in the blood, capable of causing neuritis, the endothelium of the vessels will show changes like those described by Golgi as occurring in rabies.

Still, in spite of these differences between the various sorts of neuritis, the broad fact remains that there is something about the peripheral nervous system, either its relation to trophic centres, the peculiarities of its circulation, or the laws of its nutrition, which makes it prone to disease, and the conditions of this liability are deserving of careful study. I have long been interested in what might be called a condition of latent neuritis, that state which is present during the period, sometimes a very long one, where the nerves have been exposed for a longer or shorter time for the action of some poison, or to the impaired nutrition of anæmia, and are ready to break down under any slight strain, and yet are, at any given moment, in such a condition that probably neither the microscope, nor any ordinary test could show that anything was wrong. I have recently seen a case where a gentleman, suffering from an abdominal cancer, but otherwise well, had an intense, one might almost say, malignant degenerative neuritis of the bracial plexus on both sides brought on by having the sleeves of his nightdress pinned back against the bed-pillow during etherization for an operation.

It has long been known that persons in the habit of taking alcohol to excess are more likely to suffer from the neuritis of lead poisoning. I have recently known of a case which is significant in this connection, where a boy who had been suffering from post-diphtheritic neuritis, but had nearly recovered, had his symptoms brought back by an attack of the measles. The element of proclivity on the part of the nervous system in respect to neuritis, is also shown by the fact that this is more common with some nerves than with others. For example, the musculo-spiral and ulnar nerves are oftener involved than the median, without its being possible to assign any satisfactory cause. The experiments of Bowditch, Hooper, and others, have shown that the different nerves, or parts of nerves, are affected to different degrees by ether. It is thought possible by some of the French writers that even the peripheral nervous system is not wholly

* (This whole subject has been recently worked up by Cramer (22) with great thoroughness.)

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free from a tendency to disease which comes as a part of a neuropathic inheritance. This has been worked out more especially for the case of facial palsy.

The peripheral neuritis which follows infectious processes is usually of the multiple variety, but sometimes single nerves are involved alone, or so much more than the rest that the disease of the others is thrown into the shade.

The subject of the relation of *the chronic spinal scleroses* to infectious processes is full of interest, to both the physician and the pathologist. No doubt can be entertained as to the bare existence of the practical relationship. Everyone knows how frequently locomotor ataxia and lateral and mixed scleroses follow otherwise cured syphilis, though often at a period when the signs of the primary infection had long since ceased to show themselves. The relation of disseminated sclerosis to several infectious processes and especially to malaria is almost equally evident. I have myself seen a very convincing case of the latter sort.

What is the nature of this connection? We do not know, but I think it clears the way, in a measure, to note again, the natural proclivity of the nervous system to become diseased in these ways, and especially in the way of posterior sclerosis. If we do this we shall be more inclined to be catholic in admitting other causes besides the specific virus. The case of syphilis in its relation to locomotor ataxia and other spinal scleroses is a case in point. It contradicts all that we know of the histological signs of syphilis, and the usual seat of its lesions, to assume that it is the direct cause of these degenerative scleroses, and it is delusive to try to help put this opinion by assuming an attenuated virus, or by calling to witness the rare instances of improvement under potash and mercury.

It is certainly true that gummatous inflammation sometimes gives rise to many of the symptoms of locomotor ataxia, and that this may be cured ; and it is true, also, that various modes of treatment may help ataxics for a time. But the relation of syphilis to locomotor ataxia, though I think unquestionable as a clinical fact, is not to be explained satisfactorily except on the assumption that some new and perhaps toxic influence is bronght into play, and that the nervous system is prone to suffer from posterior and other forms of sclerosis.

In passing, I cannot but stop to note the fact that locomotor ataxia may follow rapidly on what seems to be an attack of typical acute multiple neuritis. Two striking cases of this sort I have myself seen, as a result of influenza, and many are on record (compare Obersteiner and Redlich, Wiener Kl. W. schr. 1894, No. 9, p. 105). The point,

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however, on which I wish to insist is the great variety of occasional causes of spinal sclerosis, especially sclerosis of the posterior columns; causes so numerous that we cannot call any one of them, not even syphilis, specific, in any proper sense, and must recognize, as underlying them all, some peculiarities or weakness of the nervous system. Sclerosis of the posterior columns occurs as a sign of structural weakness in Friedreich's Disease;* it follows ergot and lathyrus poisoning; it occurs as a part of the degenerative process in chronic paralytic dementia; as a consequence of morphine poisoning in dogs (Alt); its outbreak is precipitated, to say the least, by such processes as influenza and trauma.†

Most important of all, posterior sclerosis is somestimes associated with progressive pernicious anæmia, and perhaps with other affections which like that—to speak of what is probable though not proved—originate in autointoxication due to morbid changes in the digestive tract.

The first communication on this subject was by Lichtheim in 1887 (VI Congress für innere Med. zu. Wiesbaden; Verhandlungen, p. 84) but, in 1893, Dr. W. Minnich published a careful monograph on the subject, and gave full reports of all the cases observed up to that time. Previously to this, and before I had seen Lichtheim's communication, I observed a series of cases, one, or perhaps two of which were cases analogous to pernicious anæmia, and in 1891 I published a paper under the title "A Group of Cases of Sclerosis of the Spinal Cord Associated with diffuse collateral Degeneration; occurring in Enfeebled Persons past Middle Life; and especially in Women." (N. Y. Jour. of Nervous and Mental Diseases, Feb. 1891.)

The clinical reports were meagre but the cases were of much interest. Various partial causes were noted, and among them lead poisoning, and chronic diarrhœa, a cause of degenerative affections to which attention has of late been called anew.

It is true that Minnich declines to admit that his cases are of the same order with locomotor ataxia or even with the sclerosis of ergot. (Tuezee, Grünfeld, Arch. für Psych. u. Nervenkrankheiten, vol. xxi.,

* What is true of posterior sclerosis is true, also, of the other spinal sclerosis and degenerations, and the mixed forms, among which latter Freidreich's disease belongs. (See a paper and discussion by Strümpell and others. Wiener Kl. W. schr. 1894, No. 4, p. 69.) Mendel here remarks that even multiple sclerosis may rest on an hereditary basis.

† (See a paper by Morton Prince, read at the meeting of the Neurological Assoc. 1894, and Klemperer, Zeitschr. für Klin. Med. XVI. The interesting observations and ingenious theory brought forward by Obersteiner and Redlich [Arbeiten; Wien, Heft II] cannot be considered as closing the discussion as to the pathogenesis of tabes, and still less of the other scleroses of syphilitic origin.

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p. 618), but he is unable to explain them on a different basis except that they seemed to originate in foci of disease* related to the vessels, and he admits them to be probably toxic in character.

His interesting paper also contains an account of another form of change in the posterior columns, which seem to occur in some cases of pernicious anæmia, probably toward the end of life, and to be due to œdema. The rest of the cord is affected to some extent. [Quite recently Bikeles has observed similar changes as following diphtheria.]

Explain these changes as we may, they help to bear out my view, that the posterior columns are a weak part of the nervous system. Lichtheim refers to other conditions besides those which I have mentioned, which contribute to disease of these tracts, and among the rest suggests the stream of sensory excitations which the tracts are called constantly upon to conduct.

It is true that just as there are differences between the different forms of neuritis, so also, the posterior sclerosis which follows syphilis is probably not identical with that due to the other causes mentioned. The ergot-sclerosis, for example, is not so malignantly progressive; and there are other peculiarities of the different varieties, some of which are pointed out by Raymond (78) and by Friedheim (38). All that I wish to maintain is that there is a strong bond of resemblance connecting them all. There is even some evidence apart from the important case of Friedreich's disease, that tabes may be a member of the so-called neuropathic family. We have all seen cases which suggested this tendency. Raymond says that it is especially prominent where the tabes develops in the later years of life.

It could not be considered in place to review here in detail all the infectious disorders with their nervous sequelæ, but a few words should be said with regard to *meningitis*, for the sake of recording some important researches. The cerebral-spinal membranes afford a remarkably favorable breeding place for many kinds of bacteria, and in proportion to the rapidity with which these organisms multiply the information tends to assume a purulent character. The symptoms are probably due, partly to the exudation itself, partly to the irritation of the sensitive membrane, partly to the absorption of toxic products. The organism which plays the largest part in the production of both the epidemic crebro-spinal meningitis,—the sporadic forms not associated with suppuration elsewhere, as well as the meningitis which sometimes accompanies pneumonia—is the so-called pneumococcus, or micrococcus lanceolatus. The history of this important bacillus has already a voluminous literature, but interesting analyses are given

* Tabes is also considered by many observers as secondary to nerve changes.

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in the papers by Netter (63), Welch (93), Foà,* and in the report of the admirable research by Flexner (37) and Barker into the nature and causes of the recent epidemic of cerebro-spinal meningitis at George's Creek Valley, Md. This form of meningitis seems to be by far the most common variety even of those occurring independently of epidemics or of pneumonia. Out of thirty such cases Netter found sixteen to be due to this organism, while the largest number induced by any other single organism was six. Out of forty-five cases of primary meningitis reported by other observers and collected by Netter, thirty were instances of pneumococcus infection.

Such cases have been repeatedly studied by the pathologists of the Harvard Medical School, and I have recently had the opportunity, through the courtesy of Drs. Forster and Wright, of examining a patient with the disease and seeing the autopsy. The whole duration of the illness was four days, and even by the end of that time the cornea of both eyes had become opaque and the conjunctiva purulent.

It is this organism which gives to normal human saliva its power of exciting septicæmia in mice. It is present in the pharynx of a considerable portion of healthy persons, but either in a harmless form or under conditions unfavorable for the invasion of the body. Netter makes the picturesque statement, for the accuracy of which I will not undertake to vouch, that "with young infants the tendency is for the infective organisms to make their way upward to the ear and cerebral membranes; with older children and adults, downward to the lungs." Nevertheless, Flexner and Barker do not find it probable that in epidemics of cerebro-spinal meningitis the micrococcus makes its way principally by the air passages. They think it more probable that it passes into the digestive tract and thence, by way of the blood, to the brain. It is interesting to note, in this connection, that the injection of cultures does not excite meningitis in animals, unless made directly into the cerebral cavity.

The high importance of both simple and purulent, and especially obstructive inflammations of the antrum, nasal sinuses, and ethmoid cells for the introduction of both this organism and others to the cerebral membranes has of late been commented on by many writers (Beck, Macewen, Dmochowski (29), Neumann (64) Schaeffer (64), and Allport (4).)

Foà and Bordoni Uffreduzzi in the course of the report of extensive experiments with the diplococcus lanceolatus (giving this name the preference over that of pneumococcus while Welch prefers to call it

* Z. Schr. für Hygiene, 1888.

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micrococcus lanceolatus), speak of the varying form and virulence of the organism, and the variety of pathological results to which it gives rise, varying from acute to chronic, according as the bacteria or the tissues get, relatively, the ascendancy.

They speak especially of these forms of pathological change; acute fibrinous, acute cell infiltration, and interstitial, circumscribed nodule formation resembling miliary tubercles. The appearance of the purulent exudation may also vary with the virulence of the organism, having, when the virus is weak, a characteristic grayish tint. It is a matter of doubt whether serous exudation is a separate form or the first stage of a purulent inflammation (Tictine). I cannot leave even this hasty reference to the bacterial origin of meningitis without calling attention to Macewen's masterly work on the Pyogenic Diseases of the Brain, in which a searching anatomical and clinical analysis is given of the paths of invasion of the organisms, the formation of abscess, and the origin and spreading of thrombosis and meningitis.

Not only are purulent meningitis and cerebral abscess excited by infection from the ear and naso-pharynx, through the introduction of the organisms already mentioned, but meningitis is also occasionally due to the bacillus of typhoid (Tictine, Adenot, Drews).

The meningitis which follows scarlatina and measles is usually due to secondary infection but may perhaps occasionally arise from primary infection.

Reference was made in an earlier part of the paper to *acute, primary, hemorrhagic encephalitis* as occasionally following various forms of the acute infectious disorders. It is not necessary to discuss the matter further except to record a possible source of error in the diagnosis, and even in the pathological examination. In a collection of cases of post-influenzal diseases reported by myself, one was included which, at the time the examination was made, I believed to be an example of this disease, but subsequent examination showed that nothing more than œdema could be made out with certainty. The case was one of considerable interest because the symptoms were so marked and so serious. The patient was an elderly lady, free from renal or cardiac disease, who was recovering from a severe attack of influenza, under the care of Dr. E. S. Boland, of South Boston. On February third, she had a brief attack of unconsciousness, without other symptoms. Three days later, in the course of the forenoon, a twitching of the left side of the face came on without loss of consciousness. After a few moments this ceased, but twenty minutes later it recurred, and this time the left arm twitched slightly. These attacks continued to occur at

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short intervals for the next thirty-six hours, until finally a complete hemiplegia of the left side came on, with loss of consciousness, and about twelve hours later, she died. The temperature is said not to have been much elevated till shortly before her death, when it reached 103° F. The post-mortem was made two hours after death and was followed by a microscopic examination. There was great œdema; the small vessels were filled with blood, and the capsular region on both sides was stained a yellowish color. On examination, I first thought that small hemorrhages were present, but finally concluded that these were artefacts, due to fracture of the softened tissue. The case seems to me worth recording here, partly because the symptoms suggested a local process of some severity, which, in fact, was absent; partly because in the accounts of the post-mortem examinations by Strümpell, Fürbringer (39), Leichtenstern, the lesions were said to be sometimes scarcely recognizable on microscopic examination, although in other cases there were extensive hemorrhages, both small and large, and often meningitis. Where meningitis is found in such instances, the question might properly arise whether we were not dealing with a secondary pyogenic infection, rather than with the effects of the primary infection alone. At all events, the lesion, with its symptoms, is an important and interesting one and deserves further study.

The diseases of the nervous system which are suspected, but not proved to be of infectious origin, are, especially, beri-beri; poliomyelitis; Landry's disease; certain forms of myositis, neuritis, myelitis; some of the cerebral palsies of children; chorea; disseminated sclerosis and other cerebro-spinal and spinal scleroses; amputation neuritis; herpes zoster.

The grounds on which a disease may be suspected to be of infectious origin are, when bacteria are found in the tissues; when the outbreaks occur in epidemics or are related to seasons or locality; when it exhibits vascular and histological changes characteristic of the infection; or specific toxicity of the blood and urine; or if it has become much less frequent since the introduction of antiseptics.

On this latter ground the intractable neuritis following amputation has been suspected to be of infectious origin (Page (70).) It is true that even when great care is taken to prevent infection amputation neuritis still occasionally occurs, but it should be remembered that surgical asepsis can be at the best but relatively perfect.

As regards the lesions to which the nervous system is liable as the result of infectious diseases, there are some which are more or less characteristic, in view of its structure and liabilities, and others which it shares with other organs and tissues of the body. Of the latter

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class are the vascular lesions, hemorrhage and exudation, which are of vaso-motor origin, or due to changes in the vascular walls such as are liable to occur in any part of the body. Where well-marked evidences of these vascular lesions occur, it is common to consider them not only as evidences of infection but also as the cause, rather than the consequence of the parenchymatous lesions with which they are associated. This argument has been used of late with confidence and ability by Goldscheider (49) in support of the theory of the infectious or at least the toxic origin of acute haemorrhagic neuritis or myelo-neuritis and encephalitis, as well as of poliomyelitis.

It is certain that in this latter disease the signs of congestion and leucocyte infiltration are prominent and a case published by myself in the Journal of Nervous and Mental Diseases, 1893, not included in Goldscheider's (40) or Siemerling's (85) lists, fully supports this view. In the report of this case, which was published in 1883 (Poliomyelitis of the Adult of two months' standing; Examination of the spinal cord, Journal of Nervous and Mental Diseases, New York, 1883), it is expressly noted that the wasting of the ganglion cells was not by groups, but "depended on their position with regard to the centre of inflammation" (p. 19), and that "the process is not one which affects ganglion cells materially more than other tissues in their neighborhood." It was also suggested (p. 24) "that the original morbid change was one affecting the vessels and the circulation." At the time this paper was written much less was known than now, about infectious processes.

Another argument in favor of the toxic theory of poliomyelitis is that this disease is certainly related to the acute (often hemorrhagic) polyneuritis, and that for this disease we find the same evidence of toxic origin but in a still more marked degree.

In both affections we see the strong vascular element in the onset, in both the occasional relation to acute infectious disease, and with neuritis the relation to beri-beri.

As regards the analogy between poliomyelitis and neuritis, several instances of the combined occurrence of the two diseases have been published (v. Gowers (43)); and the writer's experience fully bears out these views. In the case of central myelitis above alluded to as observed by myself, the clinical evidences of severe and wide-spread peripheral neuritis were unequivocal.

I can also place on record one more apparent epidemic (though a small one considering the area concerned, and perhaps for that reason unimportant) of poliomyelitis, which was observed in Boston during

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last autumn (Boston Med. and Surg. Jr. 1883, II). References to other instances of the kind are there given.

Since the publication of Goldscheider's (40) communication on the nature of poliomyelitis other confirmatory observations on the same subject have been published (Siemerling, (85).)

Although, however, the idea is generally accepted that there is a toxic cause for poliomyelitis no one has strongly affirmed that this is of bacterial origin.

As regards the infectious origin of *Landry's Paralysis*, all we can say is that it occasionally follows recognized infectious diseases, and that its abrupt onset and rapid course, often at least, unattended by gross lesions, point definitely to some toxic cause. There is no reason at present for calling this bacterial.

The theory of the infectious nature of *chorea* is interesting but still far from being proved. The subject is well presented in papers by Pianese (73) of Naples.* Dana (24) of New York, Berkeley of Baltimore and a number of authors cited by them.

Pianese's investigation dates back to 1891. The only point which needs to be mentioned here is that he found in the cervical portion of the spinal cord, the medulla oblongata, and the cerebellum—but not, it would seem, in the brain proper—a bacillus which he considers as specific and which he was able to cultivate and use for experimentation. Injections of pure cultures into animals gave rise to motor symptoms and often death, but did not cause anything that could be called chorea, so far as can be judged from the account at my disposal. In the animals experimented with the bacillus was found again in various parts of the nervous system including the brain. Pianese has also examined the blood in a number of cases of ordinary chorea, and in one of them succeeded in finding this bacillus.

Dana's (24) first article was published in Brain in 1890 and contained a report of accurate investigations into the pathological anatomy of chorea. His examinations confirmed and defined those of previous observers, among whom Handford, (Brain, 1889–90, p. 129), may be specially mentioned. It seems clear that the most obvious change consists in vascular congestion, dilatation, sometimes leading to thrombosis or capillary hemorrhage, and that wide-spread degeneration of the nerve elements, may also occur, besides sub-acute meningitis. In Dana's (24) second paper, besides changes of these sorts, (though in this case the signs of vascular congestion were less marked,) a small number of micro-organisms resembling the diplococcus lanceolatus were found in the deeper layers of the piamater and the superficial

* Original paper not accessible; abstract of some length by Eulenburg.

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part of the cortex. The case was not one of typical chorea, however, and the number of micro-organisms was too few to warrant their being considered other than incidental. Dr. Berkley's elaborate investigation showed a series of minute changes of which the chief involved the membranes and the vessels. Areas of focal inflammation were found in various parts of the brain, medulla, and pons and among other conditions which are important as indicating the infectious character of the disease, there was a peculiar endarteritis, which, as Berkley says, is rare in persons of twenty-seven, except from an infectious cause. Micro-organisms were not found, but deposits of detritus were observed which resembled the remains of bacteria. In the second part of his paper, which describes the case of a choreic dog, Berkley compares in adjacent columns the changes which he found with those given by Babes (8) as characteristic of diphtheria, in order to show the analogy between the two processes.

If ordinary chorea turns out to be of infectious origin it will be almost necessary to suppose that the bacillus which causes it is present in a harmless and inactive state in large numbers of persons, for the disease may develop almost instantly or indeed instantly in consequence of fright.

It is true that some sort of predisposition must, however, be present for—fright or not—the disease is one which occurs far more easily at certain seasons of the year than others.

As regards the value of Berkley's anatomical argument, it would be, I presume, impossible to say whether the lesions pointed to a toxic agent due to bacterial action, or to a toxic agent produced by some form of disordered metabolism or due to retained excretions.

Golgi (41) found widespread changes in chorea analogous to those seen in rabies (q. v.), i. e., such as indicated irritative lesions of the nerve elements, the connective tissue and the blood vessels, but it would certainly be premature to assert that these could only originate with a bacterial poison.

With regard to the other affections mentioned as possibly of infectious origin, I have but little to say. The argument in favor of this view is mainly that they are apt to follow other general diseases admitted to be infectious, like the acute exanthemata, but the exact significance of that argument is uncertain. One or two recent observations suggesting a relation between malaria and *disseminated sclerosis* have been published (Marie, Torti and Angelini), and I have myself seen a striking example of the sort. A man in middle life and of good health was attacked suddenly with characteristic and progressive intention tremor and scanning speech immediately after a sharp

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malarial attack. Under treatment by arsenic there was a slight improvement but the symptoms persisted.

The argument with regard to *herpes zoster* consists essentially in its tendency to occur in epidemics, though it must be admitted that this tendency is by no means strongly noticeable, and in a somewhat marked liability to follow other acute infectious diseases. (See Weigert (92), Schäffer (81), Kaposi (51), Wasielewski (90).)

The infectious theory of *epilepsy* is as yet hardly in the form to call for critical examination. The argument that the attacks are temporarily arrested by some intercurrent infectious malady, which might be supposed to furnish anti-toxines to the blood, could, one would think, scarcely be seriously advanced by any one familiar with the history and therapeutics of epilepsy. See Féré (33), Marie (60), Quériaud (96), Séglas (64), Lannois (55).

The origin of *beri-beri* is still a matter of doubt, and I will refer to it only for the sake of calling attention to some recent observations (Scheube, Glogner. Virch. Arch., vol. cxxxii., page 50), which suggest that it is a protozoal disease, analogous to malaria.

Glogner calls attention to the periodicity of the outbreaks of the symptoms, and claims to have found changes in the blood and an organism resembling the plasmodeum.

Another means by which it is thought that the infectious character of some of these nervous diseases of doubtful origin may be tested, is through the chemical and physiological study of the blood and urine. Albu (2) has made this a subject of a special paper, and many other observations bearing more or less directly on this subject have been published (Sèglas, Fisichella, Ewald, Bouchard, D'Abundo (27), Regis and Chevalier-Lavaure (79), Voison and Peron (89)). (See also Editorial with references, in the Boston Med. and Surg. Journ., Sept. 14, 1893; and Archives de Neurologie, 1893, p. 240.)

These researches have not as yet borne much practical fruit. It is evident that before much can be done in the study of morbid urine, the toxicology of normal urine must be thoroughly understood, and for this reason the recent publications of Bouchard (16) are of decided value, though the conclusion which he reaches will doubtless require modification. Bouchard maintains that if men are not poisoned by the products of their own tissue metamorphosis it is simply because the poisons, which are constantly being formed, are as constantly carried off from the blood in the urine, which is toxic to a measurable degree.

He finds seven toxic substances in normal urine, each of which has properties of its own. The first is diuretic in its action, and consists

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practically of urea. This is only toxic in a considerable dose, and is in general prophylactic in its tendency, since by increasing the quantity of urine it helps to carry off the other substances.

The second ingredient is narcotic in character and its chemical nature is not known, except that it is soluble in alcohol. The third is also soluble in alcohol and has the peculiarity of causing salivation. The other substances are characterized by their power of causing convulsions, contraction of the pupil, reduction of heat. One of the convulsive agents is potash. In uræmia all these poisons are relatively increased in quantity, and anything which interferes with elimination by the kidneys is liable to bring about this result to a greater or less degree.

SUMMARY OF FACTS AND CONCLUSIONS.

It may be instructive to compare and contrast the action of the various infective agents upon the nervous system, by way of summary, and in order to get a perspective view of the influences at work.

It is important to note that the anatomical data at our command are wholly inadequate to explain the variety of the effects produced by the different poisons.

Attention is called, (*a*) to the acute symptoms of onset of the infectious disease; (*b*) to the symptoms which occur during its course; (*c*) to the affections met with during convalescence; (*d*) to the remote effects; (*e*) to the part played by demonstrable, destructive lesions as contrasted with the effects of poisoning without gross lesions.

Variola. Toxic symptoms of *onset* often severe; no characteristic symptoms during *course.* During *convalescence* various forms of spinal disease occur with relative frequency, of which disseminated myelitis and disseminated sclerosis are the most common. Acute poliomyelitis, encephalitis and neuritis occur, though rarely. Acute ascending paralysis of the type of Landry's disease has been noted. Meningitis due to the primary infection does not occur. There are no characteristic remote symptoms, unless disseminated sclerosis be so considered.

Scarlatina. Toxic symptoms of *onset* rarely severe; no characteristic symptoms during *course,* though in some epidemics meningitis may perhaps occur (Bendel); nervous affections of *convalescence* rare but important. Optic neuritis may occur apparently without meningitis and the same may be true of epilepsy (Gowers). Meningitis, (except from secondary

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infection) is rare but does perhaps occur even in early stages (Bendel).* Focal hæmorrhagic encephalitis is reported (Fürbringer). Spinal affections and multiple neuritis are rare.

Measles. Toxic symptoms of *onset* and *course* not severe nor characteristic, though paralysis during this stage may occasionally occur (Lob).† During *convalescence*, spinal affections occur with greater frequency than after scarlatina. Optic neuritis sometimes occurs (Wadsworth),‡ but it is probably dependent upon meningitis. This meningitis is sometimes slight, and temporary. Apart from the secondary form, due to purulent infection, a primary form occurs, though rarely, the pathology of which‖ is uncertain. [See meningitis serosa; Quincke] Multiple neuritis and protracted psychoses, are rare.

Typhoid. Symptoms of *onset* rarely severe; those of *course* (and occasionally of *onset*) often severe and somewhat characteristic. The diseases of *convalescence* consist in psychoses and neuroses [typhoid spine]; multiple neuritis; neuritis of single nerves (palatine, optic, and other cranial nerves), transverse myelitis, and poliomyelitis; meningitis, both primary and secondary; disseminated sclerosis. Widespread peripheral nerve degeneration has been found by Pitres and Vaillard even when no paralysis had been noticed.

Fevers of the Malarial Type. Toxic symptoms of *onset* and *course* severe and somewhat characteristic. Symptoms of *convalescence*, multiple neuritis; disseminated myelitis and sclerosis; hemiplegia, generally due to pigment thrombosis, and other nervous affections of various types [Browning]. All of these are rare.

Diphtheria. Toxic symptoms of *onset* not severe; paralyses of *course* and *convalescence* characteristic, especially by their distribution. Lesions sometimes not found;¶ sometimes widespread, and involving the cord and medulla as well as the nerves. Bikeles has found changes in the posterior root zones in an ataxic case. Disseminated sclerosis has been observed as a sequel, though rarely.

* Wiener Med. Presse, 1894, No. 23, p. 889.

† Cbl für Klin. Med. 1893, 50.

‡ Boston Med. and Surg. Jr. 1880, vol. 103, p. 636.

§ Medical Ophthalmology.

‖ Sammlung Klin, Vorträge, No. 67, 1893.

¶ See, among recent pathologists, Bikeles in Heft II of Obersteiner's Arbeiten. Wien, 1894.

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Tetanus. Typical complex of symptoms, which however do not come on at once after inoculation. This interval of latency is not occupied by bacterial growth, since it occurs after injection of sterilized cultures. Brunner thinks there is a gradual increase in the irritability of the spinal centres. Lesions sometimes occur in the spinal cord, but are not necessarily coextensive with the symptoms. There are no characteristic affections of *convalescence.*

Rabies. Typical complex of symptoms, which are probably toxic in the strict sense, though associated whether (regularly or not is uncertain), with slight widespread lesions (Golgi) involving the cord, medulla, cerebellum and brain-hemispheres. There are no characteristic affections of *convalescence.*

Influenza. Toxic symptoms of *onset* often severe. It is rare to see characteristic nervous symptoms during *course*, though optic neuritis and other nervous affections occasionally occur very early. Affections of *convalescence* numerous and important; the chief being, multiple neuritis; myelitis, especially of the cervical region; meningitis and encephalitis (focal, hæmorrhagic); protracted psychoses; severe neuralgias; papilitis and retro-bulbar neuritis (rare); locomotor ataxia, perhaps usually preceded by multiple neuritis. It is doubtful if, in the case of locomotor axtaxia, influenza is more than a contributive cause.

Erysipelas. No characteristic toxic symptoms of *onset* and *course.* During *convalescence* (though rarely), meningitis, myelitis, multiple neuritis.* A few cases of optic neuritis and atrophy (v. Gowers) have been observed even quite early, and a case of paralysis of the soft palate.

Pneumonia. Toxic chill at *onset.* During *convalescence*, meningitis multiple neuritis;† optic neuritis (Gowers).

Acute Rheumatism. Symptoms of *onset* not severe. During *course*, hyperpyrexia, meningitis (rare from primary infection), delirium coma, convulsions. During *convalescence* chorea, neuritis.

Syphilis. No characteristic acute toxic symptoms, strictly speaking, unless neuralgia be so considered. Eventually, typical cerebral, spinal, and peripheral affections due to lesions originating in new growths. Remote effects, of which the pathogenesis is undetermined, consisting in diffuse dorsal myelitis

* See, among recent writers, Lew; Charité Annalen, xv. 1890.

† See Ross. I have seen a typical case during the past winter.

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approaching the type of the non-syphilitic form; also locomotor ataxia, lateral and amyotrophic-lateral sclerosis; perhaps nuclear degenerations; general paresis.

Gonorrhœa. No characteristic toxic symptoms of *onset.* During *convalescence*, myelitis (rare) of unknown pathogenesis.

Lepra. No characteristic toxic symptoms in a strict sense, though the blood and urine are said to contain toxines. Nerve-degeneration excited by presence of bacillus in lymph spaces of nerve and in the nerve-fibres. (Babes.)

Tuberculosis. No strictly toxic symptoms, unless the paralyses and cerebral disorders occurring during tubercular meningitis are of that character, which is probable. In the course of the disease, local affections of the brain, cord, and nerves occur, of which meningitis and neuritis are the chief.

Ulcerative endocarditis (due to various bacteria) is associated not infrequently with meningitis.

Pyæmia and Septicæmia, including the puerperal form. Violent toxic symptoms of *onset* and *course.* During *convalescence* multiple neuritis (rare).

Beri-beri, and *kindred affections;** *Landry's disease; Poliomyelitis;* some forms of *multiple neuritis* occurring independently of antecedent disease; *chorea; herpes zoster; amputation neuritis* may be classed as possibly of infectious origin. The evidence for the infectious origin of *epilepsy* and *paralysis agitans* is at present of little weight.

It is important to separate (A) the symptoms and lesions which we can assert to be directly due to the specific toxines of the various infectious diseases, from (B) the effects of anæmia, œdema, intoxication from disordered metabolism, on the one hand; and on the other hand, from (C) the affections which spring into existence because the nervous system was prone to them, either from peculiarities of structure, anatomical surroundings, circulation, and the like; or because other causes had been at work to which the infective poison only contributed new effectiveness for evil.

In the first class (A) we can fairly place the symptom-groups of tetanus and rabies; and with reasonable probability the chill, pains, paræsthesias, acute delirium, disorders of micturition, and other symptoms, which characterize the outbreak of many acute febrile disorders, though it must be admitted that we know little or nothing positive about their pathogenesis.

* Epidemics among fishermen on Northern coasts.

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The focal, hæmorrhagic encephalitis and myelitis occasionally observed after influenza and other infectious processes may be due to the action of the specific poison on the tissues, and the vessels. Acute poliomyelitis and acute hæmorrhagic neuritis may be of the same origin, though these opinions need further substantiation.

As regards tetanus and rabies, it is not probable that the anatomical lesions which have been found after death (Golgi and others) are actually the anatomical substratum of the characteristic symptoms. They may have been simply of correlative origin.

Post-infection meningitis is generally of purulent character, and implies the presence of micro-organisms, but the non-purulent form bears a relation to infectious disease which is need of further study.

The paralyses of diphtheria are probably attributable to the direct action of the poison, but, as in the case of tetanus and rabies, it is by no means certain that the lesions sometimes found (though often missed) are the true cause of the symptoms. It is more probable that the first and characteristic effect of the poison is to excite a chemical and molecular change, and that this alone is strictly characteristic. Sometimes the paralysis passes away too rapidly to be attributed to neuritis.

The lesions, when present, may be many and various, and the neuritis, which is the commonest form, is of varying types.* The lesions met with in meningitis, and in a certain sense those characteristic of tuberculosis and syphilis, are directly due to bacterial action.

In the following cases the influence of Cause A is more or less combined with those of Causes B and C:

The question as to the origin of the post-infectious multiple neuritis is one of great interest. The most obvious position to take is in favor of its direct dependence on the infective poison, but in view of the general fact of the intolerance of the peripheral nerves to poisons of all sorts it is fair to suspect that the post-infectious neuritis may sometimes be due to toxic substances developed from impaired metabolism and sometimes to anæmia and œdema (Class B).

It appears to be occasionally possible to induce neuritis by experimental inoculation with diphtheritic cultures, and the form and sequence under which the diphtheritic paralyses clinically occur suggests the action of a specific poison. If, however, the forms under which neuritis typically occurs are considered, it becomes obvious that natural weaknesses of the nervous system are at least contributive causes.

It has been suggested that alcohol and pressure are at least partial

* See, among others, Paul Meyer's paper in Virchow's Arch., 1881, vol. 85, p. 181.

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causes of the degenerative neuritis which follows such fevers as typhoid.* These influences may doubtless act as contributive causes, but the nerves must have been, antecedently, in a very susceptible state, closely allied to neuritis or identical with it.†

The variety of conditions under which the spinal sclerosis,—both disseminated and system scleroses,—occur, suggests predisposing tendencies of one or another kind, common to all, and acting in addition to the various special causes.

The recent observations of Obersteiner,‡ and that the narrowing of the posterior nerve-roots at their point of penetration through the pia mater suggests a mode by which slight pressure (as from simulated meningitis) could set up the ascending degeneration characteristic of tabes, do not afford a complete explanation of that disease. This theory leaves unnoticed the close analogy between the syphilitic tabes of the lateral columns and that of the posterior columns. The occurrence of the peripheral neuritis, met with in connection with syphilitic tabes, but rarely seen at other stages of syphilis, points to the presence of some poison as yet undiscovered, and strengthens the probability that the posterior-column degeneration of anæmia and ergot poisoning, and that of syphilis, are mainly of toxic origin.

The complex nature of the infective poisons is shown by the varying relation between the toxic symptoms of the first stages (fever, delirium, pain, etc.) and those of the later stage.

In influenza, the fever may be of trifling amount and duration, yet the post-infectious diseases severe and lasting; in cerebro-spinal meningitis, the fever is often slight. Again, it has been found possible, experimentally, to neutralize to a certain extent the fever-producing element in bacterial poisons by special anti-toxines.

Finally, it is probable that a routine examination, by the best modern methods, of the central and peripheral nervous system, after infection, would show changes to be present far oftener than is now supposed. The studies of Golgi in rabies lend support to this view.

As an additional contributive cause of lesions, fatigue may be mentioned, since the experiments of Hodge show that this is capable of exciting manifest changes in the nerve cells. It is a question for further investigation whether or not fatigue acts more forcibly on nerve centres poisoned by the products of infectious processes.

*See a discussion on post-typhoidal paralysis; Trans. of Assoc. of American Physicians, vol. iii.

† Pitres and Vaillard find signs that nerve degeneration is common in typhoid, independently of paralysis.

‡ Arbeiten aus dem Institut für das Studium des Nervensystems, etc. Wien. Heft II, 1894.

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THE RELATIONS OF INFECTIOUS PROCESSES TO MENTAL DISEASE.

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NEARLY a century ago Benjamin Rush, when considering the etiology of intellectual derangement, declared that gout, dropsy, consumption, pregnancy and fevers of all kinds produced madness by acting upon the brain in common with the whole body; and in one or another way before his time and since, the subject assigned to me in the opening of this discussion has claimed the attention of the medical profession; it is one aspect of the old humoral doctrine of disease in general, to which we now recur, aided by experimental research, and not simply by clinical and pathological observation.

Are special types of mental disease ever due directly to infection? Are the mental disorders of well-known infectious diseases always toxæmic, or are they sometimes the result of toxæmia and sometimes of anæmia or exhaustion? Are particular types of insanity due to the action of specific micro-organisms? In other words, has acute mania a bacillus which belongs to it, and to it alone; has melancholia its special pathogenetic microbe; is a special type of confusional insanity of microbic origin; has general paralysis of the insane a *bacterium sui generis?* These questions lead us naturally to consider inferences and conclusions drawn, 1, from clinical and clinico-pathological observations; 2, from the analogies with nervous affections, not psychoses, but which are known or believed to be of microbic origin; 3, from the results of combined clinical, pathological, and bacteriological observations and investigations.

Clinical and clinico-pathological observations have furnished us with a literature comparatively voluminous, but for the purposes of this discussion its teachings can be compressed into a small space. Two valuable contributions are a paper by Hurd* on "Post-febrile Insanity," and a report made to the Congress of French Alienists, in August, 1893, by Drs. Regis and Chevalier-Lavaure† on "Auto-Intoxication in Mental Diseases."

* Read before the Medical and Chirurgical Faculty of Maryland, April 28, 1892. Amer. Jour. of Insanity, July, 1892.

† Summarized by Parant in the American Journal of Insanity, January, 1894, vol. 1, No. iii, p. 423.

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Regis and Chevalier-Lavaure refer to many cases of insanity in which neither heredity, organic disease of the nerve centres, nor any of the disturbances designated as neuroses can be assigned as a cause. They attribute the mental disorder in these cases to the excessive production or storing of products which are usually accumulated in much less amount or are eliminated. The blood, organic liquids, and especially the urine, undergo various modifications, their composition changing with the type of mental disease. They distinguish mental disorders as due 1, to infectious diseases; 2, to visceral disturbances; and 3, those associated with diathetic maladies. The second class includes mental disorders of hepatic and renal origin. They refer to the importance of studies of the toxicity of the intestinal contents, which recalls the important researches of Herter* on the relationship of the toxic products of intestinal putrefaction to epileptic seizures, presented in a paper read before the American Neurological Association in 1892. In the third group of insanities dependant upon auto-intoxication are included those occurring under the influence of such diatheses as arthritism, herpetism, diabetes, cancer, etc. Regis and Chevalier-Lavaure query whether or not general paralysis of the insane is not sometimes due to infection, and Parant, commenting on this suggestion, remarks that we sometimes see cases of mental confusion ending in a progressive paralytic failure, with delusions somewhat different, but with other symptoms of general paralysis, and that, therefore, while this opinion is not susceptible of any great generalization, it is nevertheless based upon actual facts.

With Hurd, I believe that we should recognize the fact that mental disorders described under the general head of confusional insanity—and I might add other types and subtypes—should be regarded as developing either from specific poisoning, from shock, or from anæmia and nervous exhaustion. Under the head of insanity from specific poisons Hurd comprises, "the delirium of fevers, both intermittent and exanthematous, of pneumonia, of uræmic poisoning, the transient insanity of influenza, the mental confusion of multiple neuritis, the delirium of iodoform, salicylic acid and chronic alcoholic poisoning, and the delirium of puerperal fever. In this group of cases we have a poison acting directly upon the central or peripheral nervous system, producing an intoxication as shown by incoherence of ideas, confusion and a more or less rapid flow of thought. This poison may be of the nature of uræmia or of some toxic albumens—which Welch has shown to be produced in pneumonia and diphtheria, which possess a special toxicity to the nervous system—or an intoxicating drug which has a

* Herter: Journal of Nervous and Mental Disease, August, 1892, vol. xvii, n. s. p. 644.

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prolonged action. These poisons produce an active delirium which is generally self-limited and disappears when the exciting cause is removed and the period of convalescence established." While fully admitting the development of insanity from specific poisons, Hurd believes, however, that mistakes have arisen by confounding the confusion of febrile delirium with true post-febrile insanity. As the essential feature of delirium is mental confusion, and as delirium is often the product of toxæmia, well illustrated by drug intoxications, this febrile delirium at least is toxic in origin; the insanity which follows may sometimes be due to persisting toxæmia, but in many cases it is the result of shock, anæmia, or nervous exhaustion.

Between 1834 and 1865, Chomel, Esquirol, Simon, Sauvet, Thore and Baillarger—cited by Hurd—described the mental disorders that accompany typhoid and other forms of fever, and among later writers Clouston,* Wood,† and Tuke and Woodhead,‡ have especially considered the subject. Febrile and post-febrile insanities have been shown by Nasse§ and Spitzka‖ to be of not rare occurrence in children; the fevers, during or after which they develop most often these mental affections, being typhoid, scarlet fever, measles, rheumatism, and diphtheria. Clouston and Wood both regard post-febrile insanity as due to the anæmia and exhaustion which result from acute infectious disorders; and the latter has proposed to include under "confusional insanity" a special group of cases following infectious diseases, injuries, operations and other causes. Korsakoff¶ regards the mental confusion, or confusional insanity described by Clouston, Wood, Hurd and others preceding them, as essentially a toxæmia, due to a special poison such as is developed in multiple neuritis, influenza and other infectious disorders. According to Tuke and Woodhead, in the acute maniacal delirium which occasionally presents itself during the course of infective fevers, the two factors which should be taken into consideration are, first, the specific poison which appears to act directly on the nerve cells, giving rise to stimulation and impaired nutrition and consequent granular degeneration; and secondly, the high temperature during the persistence of which metabolic, or to speak more accurately catobolic, processes go on rapidly. In very acute cases extraordinary proliferation of connective-tissue cells, round the vessels, and migration of leucocytes take place, this

* Clouston: Clinical Lectures on Mental Diseases, 1884, p. 416.

† Wood: University Medical Magazine, Dec. 1889, vol. ii, p. 117.

‡ Tuke and Woodhead: Tuke's Distionary of Psychological Medicine, vol. ii, p. 911.

§ Nasse: Allgemeine Zeitschrift für Psychiatrie, Berlin, 1870, p. 11.

‖ Spitzka: Keating's Cyclopedia of the Diseases of Children, vol. iv, p. 1046.

¶ Korsakoff: Allgemeine Zeitschrift für Psychiatrie, 1889, Bd. H, 4, p. 475.

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being a condition commonly associated with the presence of micro-organisms, and well-marked in acute exudative meningitis.

Numerous isolated cases of acute delirium, acute mania, acute confusional insanity so-called, and other forms of acute mental disease variously designated, occurring in connection with fevers like typhoid, have in recent years been reported in medical journals, but I can only here refer to a very few of these.

An argument in favor of the mental disorders of infectious diseases being due to toxæmia rather than to anæmia or exhaustion is furnished by the well-known fact that such diseases are sometimes ushered in by violent mental manifestations, or that such manifestations are the chief features of the disease, which pursues a course which is largely afebrile. I have seen and recorded several cases of typhoid fever which began or were preceded by an acute delirium, which closely simulated, and in some cases was diagnosticated, as common acute mania. A form of afebrile typhoid fever with severe cerebral symptoms has been recorded by Gerloczy :*

A girl, nine years old, was taken sick with headache, chill and fever, and soon became noisy and restless, and eleven days after the onset was comatose and screamed and tossed incessantly. Meningitis was suspected ; the temperature never rose above 39° C. The treatment consisted of ice cap and small doses of chloral at night. On the seventeenth day of the disease she became more quiet, so that an examination was possible, and this among other things showed a swollen spleen. From this time a gradual improvement took place. On the twenty-fifth day the child became conscious, and from the twentieth to the thirtieth numerous furuncles developed. Convalescence was interrupted by a rubelous eruption without fever, but otherwise progressed favorably.

Chaslin † has described a form of mental disorder usually acute, which is neither mania nor melancholia, which may be attributed to a rapid depression of the nerve centres from infection or auto-intoxication, and which he believed to be an intermediary form between functional psychoses and insanities with well-marked and profound lesions ; although it assumes often the character of a true disease by the somatic phenomena. It is characterized by confusion of ideas, perception, and personal apperception. It may or may not be accompanied by hallucinations ; motor agitations, depression, or stupor may be present. The disorder has a great analogy with the delirium of

* Gerloczy: Deutsche med. Wochenschr., April 14, 1892, No. 15, p. 328.

† Chaslin: Annales Medico-psychologiques, Paris, 1892, p. 225. Cited in La Sémaine Médicale, Paris, August 10, 1892.

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chronic intoxication, and has been designated under the name of "mental confusion." Cases similar to Chaslin's have been recorded by others. Liebermeister has attributed the great depression of temperature in such cases to the action of a virus—microbic or chemical—on the heat-regulating centres. The insanity of sunstroke is probably dependent upon a toxic condition, and by some is believed to be the result of catabolic changes produced by high temperature.*

The somatic and psychical cerebral symptoms which accompany these infectious febrile disorders are not infrequently attributed to meningitis, or even encephalitis, and the patients are sometimes treated on the supposition that they are suffering from intra-cranial inflammatory affections, which are indeed occasionally present. These diagnoses are seldom corroborated by autopsies, and with our present lights the evidence—clinical and pathological—points rather to toxæmia.

Lloyd† and Tull‡ have reported cases of acute delirium without recognizable cause, leaning to the view that many such cases are infective in nature. The temperature in Tull's case, also seen by Lloyd, before death rose to 103.4° F. The autopsy revealed an uncertain incipient meningitis. Much negative pathological evidence in favor of the toxæmic origin of acute delirious seizures, like that afforded by the case of Lloyd and Tull, might be adduced, as many cases have been reported in which the autopsies were practically negative in their findings. My own experience has afforded several such cases, one of which is recorded in another paper.§ I assisted at the autopsy of this case in the post-mortem-room of the Philadelphia Hospital:

The woman, a patient of Dr. E. P. Davis, three hours before she was brought to the hospital by the police patrol, had been delivered of a child. She was in a state of acute mania, and her limbs were extremely hyperæsthetic, with active contractures, and a scattered petechial eruption. The symptoms were suggestive of a cerebro-spinal meningitis with diffuse neuritis; but the autopsy showed clearly that no meningitis was present and doubtless the case was one of toxæmia associated with anæmia.

The modifying influence which it is claimed is exerted by certain infectious diseases on mental disease is worthy of passing notice in

* J. Batty Tuke and German Sims Woodhead: Tuke's Dict. Psch. Med., 1892, vol. ii. p. 911.

† Lloyd: Medical News, Jan. 31, 1891, vol. lviii, p. 122.

‡ Tull: Ibid, p. 127.

§ University Medical Magazine, May, 1893, vol. v, p. 611.

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view of the present ideas regarding toxines and anti-toxines. Mayberry* has referred especially to the influence of facial erysipelas in cases of melancholia; and Dinter, cited by Mayberry, has reported two cases of melancholia with erysipelas, one of which went on to recovery, and the other was improved only for a single day. In an epidemic of forty-four cases of facial erysipelas, Mayberry gives the results of the disease upon the mental condition as follows: "Three were cases of acute melancholia, one of which went on to rapid recovery, one slowly improved and one remained unchanged; the two former were simple melancholia, and the latter melancholia agitata. Nine were cases of chronic melancholia, two of whom were temporarily improved; in one, a case of melancholia agitata, the delusions were more marked and the agitation greater, and six remained unchanged. One was a case of stuporous insanity, which seemed to begin to improve from the date of the sickness. Four were cases of acute mania, all of whom remained unchanged. Eleven were cases of chronic mania, one of whom had a lucid interval, the other ten were not appreciably affected. Three were cases of paretic dementia, two of whom showed greater deterioration, and one remained unchanged. Two of epileptic mania remained unchanged. One of senile dementia became mentally weaker. Ten of terminal dementia showed no change." From these and similar observations on other cases Mayberry was led to believe that erysipelas is more likely to influence favorably depressed cases of insanity of a comparatively short duration.

To the consideration of the analogies of certain insanities with affections of the nervous system, not psychoses nor insanities, but which are known or believed to be of microbic origin, I shall give brief space, as this aspect of the discussion has necessarily been largely treated of by my colleagues. In the affections which are now claimed as microbic, some with a strong showing of facts and arguments, and some with evidence as conclusive as can be furnished by medical science, are now included multiple neuritis, some forms of myelitis, tetanus, tetany, Landry's paralysis, chorea, epilepsy, disseminated sclerosis, and paralysis agitans. Nearly all of these affections have or may have mental disorders associated with them.

The mental disorders of multiple neuritis have been frequently recorded. Several years ago † I called attention to the concurrence of multiple neuritis with both myelitis and encephalitis, but I believe now that most of these cases with mental symptoms were instances of

* Mayberry: Journal of Nervous and Mental Diseases, May, 1894, N. S., vol. xix. p. 298.

† Mills: Journ. of Nerv. and Ment. Dis., 1887, vol. xiv. p. 209.

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toxæmia rather than encephalic inflammation. Fisher* has reported a case of multiple neuritis in which loss of time and place were marked symptoms, a similar condition to that which occurs in drug toxæmias. Since the recent series of influenzal epidemics first appeared I have seen many cases of acute and chronic mental disease directly or indirectly its offspring, and often associated with neuritis. A few of these have been fatal cases of acute mania, during or following closely upon the heels of the influenzal attack, but many have been chronic and milder forms of insanity or of mental disorder, not rising to the dignity of true insanity. To most of these I have referred more at length elsewhere.†

The microbic origin of some forms of myelitis with which mental affections are occasionally associated is now generally admitted; and choreic insanities with mental manifestations equalling in violence the motor disorder are well known. Regis‡ has reported a case of choreic insanity with a form of delirium which seemed to be closely akin to that of alcoholic delirium. Dana§ has reported an interesting case, with autopsy and bacteriological investigation, in which was found a micro-organism closely resembling the diplococcus lanceolatus, which, he believes, gives weight to the belief that in some cases at least a microbe produces the disease.

Marie,‖ in his great work on *Diseases of the Spinal Cord*, has expressed his profound conviction that the most efficient cause of disseminated sclerosis is infection, or rather *infections*. As early as 1884 he asserted in categorical fashion this relation of cause and effect, having collected twenty-five cases in which disseminated sclerosis had followed acute diseases; of these cases a certain number had been described in 1879 in a memoir of Kahler and Pick. Marie holds that in certain infectious maladies symptoms of central nervous affection may develop during convalescence, and later may amend and disappear, but sometimes sclerosis, and especially disseminated sclerosis, arises with greater or less rapidity. He does not contend that disseminated sclerosis has its special microbe, but that this condition may appear in a crowd of infectious diseases which differ very much, one from the other; and that it is most frequently the result of one of the combined infections so frequent in the course of different infectious

* Fisher: N. Y. Medical Journal, Feb. 13, 1892, vol. lv., p. 186.

† Mills: Trans. Phila. Co. Med. Soc., June 13, 1892, and Medical News, Jan. 30, 1892, p. 118.

‡ Regis: Journ. de Médicine de Bordeaux, July 20, 1890.

§ Dana: Amer. Journ. Med. Sci., Jan. 1894, vol. cvii, No. 1, p. 31.

‖ Marie: Leçons sur les Maladies de la Moelle, 1892, pp. 140–142.

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diseases. The infection is not so much due to a special microbe as it is to the localization of the infectious agencies in the nerve centres. If we accept the view of Marie regarding disseminated sclerosis, the same reasoning would lead us to believe that other forms of sclerosis and general paralysis of the insane—and perhaps neuro-systemic degenerations in general—are due to infection. Marie's * observations on the poliomyelitic origin of the lesions of the white medullary fasciculi in pellagra, general paralysis, and different combined scleroses, are of great importance in this connection.

Passing to the consideration of the results of combined clinical, pathological, and bacteriological investigations, the most valuable contribution which has thus far come from an association of clinical and laboratory work is an article entitled "A Contribution to the Etiology and Pathogenesis of Acute Delirium," by Dr. Carlo Rasori,† assistant physician to the Hospital for the insane, at Bologna, the work emanating from Tizzoni's Laboratory for General Pathology.

As Rasori's paper is of great importance to our subject, and as it has not received the attention it deserves from the medical journals of this country, I shall refer in detail to its results.

Rasori describes in detail a case of acute delirium, with autopsy and the results of a careful bacteriological examination. At the outset he defines what he means by acute delirium, referring to the published work on this subject of Prof. Bianchi,‡ who has compiled the literature of the subject. A study of Bianchi's article shows, according to Rasori, that while authors agree upon the clinical picture and anatomico-pathological findings, their opinions with reference to the etiology and pathogenesis are often different and contradictory, and Rasori believes these differences are largely due to confounding different disorders and to the existence of transition forms, the most varying types of insanity having clinical episodes which sometimes give the picture of acute delirium. Acute delirium, as accepted by Rasori, is based upon Bianchi's descriptive definition—a general affection of the brain cortex which leads to a rapidly increasing and soon culminating dissolution of the psychical personality; with varying but severe disturbances of the senses and active excited movements; with quiet periods

* Marie: La Sémaine Médicale, Jan. 13, 1894, p. 17.

† Centralblatt für Bakteriologie und Parasitenkunde, October 20, 1893, Bd. xiv., No. 16, Jena. p. 509.

‡ Bianchi: Sull' Origine Infettiva di una forma di Delirio Acuto per Prof. L. Bianchi e Dott. F. Piccinino (Estratto dagli Atti della R. Acad. Med.-chirurg. di Napoli, anno lxvii., Gennaio, Febbraio, N. S., No. 1.) Napoli (Stab. Tipogr. A. Tocco. S. Pietro a Maiella 31), 1893 Reference cited from Rasori's article, as I have not access to the publication of Bianchi.

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almost to the absence of consciousness; with intermissions of calmness; with convulsions, fever, early emaciation, rapid coma, and usually a fatal termination in from one to three weeks.

Rasori's patient was a woman, forty-five years old, without previous history of any moment. Her attack came on with persistent headache. She became confused and excited, talkative and incoherent, with a tendency to purposeless actions and to destructiveness, and refusal to take food. Her face was congested. She became noisier and more confused, consciousness was much disturbed, and she seemed to be the victim of terrible visual and aural hallucinations. Both pupils were sluggish and large, the right a little larger than the left. She had no fever, but she grew rapidly worse, developing opisthotonus, with clonic spasms of the facial muscles; noisy respiration, difficult deglutition, retention of urine, and rapid symptoms of exhaustion. Her temperature at the beginning was 38.5° C. On the evening before her death it reached 39.6° C. She died two weeks after the first appearance of the mental symptoms.

The autopsy was conducted with the strictest antiseptic and bacteriological precautions. With a sterilized syringe fluid was removed from below the dura, and a part of it deposited in a sterilized tube of obliquely congealed agar, and these tubes were placed in a thermostat at about 35° C.

Marked congestion was present in the meninges and brain substance, also œdema and several hemorrhagic spots below the membranes. In the right temporal lobe the cortical substance was softened. The lungs were emphysematous, the lower lobes showing hypostasis and œdema, and an adhesive pleuritis on the left. The colon had areas of congestion.

Two inoculations with subdural fluid led to the development of the same micro-organisms, a small bacillus with rounded ends, three times as long as its width. Usually it appeared alone, but sometimes two or three were found hanging together by their ends. Besides the typical forms, others longer and either straight or twisted were present; these took the stain less readily. Rasori made numerous experiments and found that the micro-organism grew well in different culture media. His experiments with gelatine, bouillon, and agar cultures are given in detail. He was not able to say anything concerning the influence of physical or chemical agents upon the vitality of the micro-organism or the possibility of its development in amœbic conditions; he was only certain that its life was of short duration if the culture medium was not the proper one. He made a series of four inoculation experiments upon rabbits to establish the pathogenetic effect of the micro-organisms. The inoculations were made as follows: 1. Below the dura; 2. Below the skin; 3. Into the nose; and 4. A subcutaneous inoculation of a culture of blood agar obtained from the second experiment. Rigid antisepsis was employed.

The experiments demonstrated that the bacillus obtained from the subdural fluid of the patient grew and multiplied in the body of the rabbit, and produced a toxic substance which destroyed the animal, with symptoms of acute septicæmia, at a period varying from one and a half to six days.

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The post-mortem findings of the animals were: Hyperæmia of the meninges, subpial hemorrhages, staining of the lungs, liver, and kidneys, and swelling of the spleen. Preparations were taken from the brain and spinal chord which showed, in addition to nervous elements and leucocytes, rich cultures of the kinds used in the inoculations.

"It would be premature," says Rasori, "to draw positive conclusions from so small a number of experiments about the special conditions which favor the natural infection, as well as about the most likely way to permit this micro-organism to enter the human organism, where it there exerts its influence, especially upon the central nervous system, as appears from the picture of this disease, which is usually fatal, according to the most prominent alienists. I reserve the privilege of completing in due time the investigations contained in this communication, and also of announcing the results of the histological observations which I shall make in the future."

In August, 1893, in consultation with Dr. Charles S. Potts and Dr. J. F. Berlet, of Philadelphia, I saw a case of acute delirious mania, a report of which has been made to the Philadelphia Neurological Society by Dr. Potts.* The patient, a professional man, thirty-three years of age, in the preceding June had been attacked with uticaria, lasting until August when a carbuncle made its appearance on his upper lip. This was opened and curetted. For several days prior to the operation he had taken rather large quantities of whiskey and morphine; but these were stopped, and he did well for nine days, when he became violently delirious with hallucinations, delusions, incoherence, great agitation and excitement. His temperature remained normal until three days after the attack when it rose to 101° F., and continued to increase until three hours before death, when it reached 108.2° at which time delirium gave place to coma. The autopsy, made seventeen hours after death, by Dr. J. W. McConnell, in the presence of Drs. Potts, Berlet, Klemm, and the writer, was practically negative. Cultures were made from the cerebro-spinal fluid by Dr. D. Braden Kyle, and demonstrated the presence of the so-called streptococcus lanceolatus, or pneumococcus of Fränkel and Weichselbaum, and also the staphylococcus pyogenes aureus and albus. Microscopical examination of the cortex showed peri-vascular exudation and leucocytes in the lymph sheaths and peri-gangliar spaces. Dr. Potts, in reporting the case, suggests that as the germs isolated were those usually found in meningitis, the lack of microscopic findings was probably due to the fact that the toxæmia was so violent as to cause the death of the patient before naked-eye appearances had time to develop.

Through the kindness of Dr. M. V. Ball, of Philadelphia—to whom I am indebted for assistance and valuable suggestions in the preparation of this paper—I am able to present the report of an unpublished case of acute delirium with autopsy, and the results of a careful bacteriological examination.

* Potts: Journal of Nervous and Mental Disease, May, 1894, vol. xix., N. S., p. 338.

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The patient, a German, fifty-five years old, with a negative previous history, except of gonorrhœa and occasional rheumatic attacks, had been confined in prison for nearly eighteen months. He was suddenly attacked with delirium, having previously been to all intents and purposes in the best of health. He had marked delusions with hallucinations, and believed that people were coming to attack him. In his frenzy he tore up the furniture of his cell, destroyed his own property, and threatened death to any one who would enter his cell. It became necessary to overpower him, but this was done as carefully as possible; and he was seen by Dr. Ball an hour after he had quieted down. He recognized the doctor, said he felt dizzy, and that he believed this was due to a fall from his carriage some years before. Every now and then he would stoop down as if to pick up something, apparently because of hallucinations of sight. His whole appearance was of one in a fit of alcoholic delirium, but he had not had any liquor for eighteen months. He was given a mixture of hyoscyamus and bromide, and next morning seemed to be in a state of unnatural gladness. He had no fever, ate his food, and made no complaints; but he had said to his keeper, "If they are going to take me out to hang me they had better do so before breakfast"—clearly indicating delusions of persecution. In the evening he was quiet, but the next morning he was found hanging to the door of his cell having strangled himself with the band of his pantaloons. When found he had evidently been dead several hours. The autopsy revealed under the dura several fresh patches of exudation; the pia was glassy and covered with two or three spots of a greenish white exudate; the ventricles did not contain an unusual amount of serum. Three small round cysts filled with a milkish colored gelatinous fluid were found in the choroid plexus on each side. Cultures were obtained from the ventricle serum and from the contents of the cysts.

The following is an extract from Dr. Ball's bacteriological report:

"In both cases in two days a fine growth occurred along the needle track in a gelatine agar tube—a sparse whitish growth on the surface of the tube, not liquefying. The germ obtained was a very small bacterium arranged in twos and threes, resembling micrococci very much, but plainly larger in one diameter than in the other. Most curious was the growth or rather formation of needle-shaped crystals along the growth of the germ. The fine white, six-sided crystals arranged themselves in bundles and stars, giving a growth of most characteristic appearance. From the original growth many cultures have been obtained, giving rise to the same peculiarity. In gelatin alone, however, it has not been obtained, but in pure agar and in gelatin agar there has been no difficulty. A chemist who made an examination of the crystals believes them to be a sulphate of some kind. I had an idea that it was a nitrite, but the reaction for nitric acid or ammonia was not obtained. The crystals can be plainly seen with the naked eye. What relation they bear to the growth is, of course undeterminable without further study. The bacillus of lactic

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acid is the only germ on record which gives rise to crystals along its culture."*

I am indebted to Dr. James R. Hunt, interne of the Hospital of the University of Pennsylvania, for the notes of another unpublished case of considerable interest. The patient was under the care of Dr. John Ashhurst, Jr., in the surgical wards of the University Hospital, who has kindly permitted me to refer to the case in this connection.

The patient was a German laborer, twenty-six years of age, who presented a history of obscure renal disease extending over a period of six years. A distinct resistance was felt in the region of the right kidney, which was quite tender on pressure. The urine contained numerous pus-cells, but no blood and no crystals. An exploratory operation was performed.

A needle was thrust into the kidney-substance in various directions in search of a calculus, but with negative results. A large drainage-tube was placed in the wound, which was closed. The day following the operation the patient became unmanageable, tearing and displacing

* The preparations and procedures necessary for a bacteriological examination of the brain have been kindly summarized for me by Dr. Ball. The materials necessary to be at hand are a platinum needle. clean scissors, three nutrient agar tubes, three nutrient gelatine tubes, three nutrient blood-serum tubes, three nutrient bouillon tubes, a one per cent. sublimate solution, and an alcohol lamp or Bunsen burner.

The precautions necessary are: 1, that the tissue must not be exposed longer than absolutely necessary; 2, the instruments and needles must be passed through flame before touching the tissue; 3, the tissue must be at once conveyed to the nutrient or medium, and in doing this every contamination must be avoided—the cotton plugs, for instance, when removed from the tubes must be singed before they are taken out, and before they are replaced; 4, if the nutrient material is fresh, one tube of each kind must be kept for control experiments; 5, have labels on the tubes so that they can be marked at once.

After the calvarium has been removed, wash the dura with the sublimate solution, then with the scissors—which have been sterilized in the flame—lift off a small portion of the dura, and if any subdural fluid is present, with a sterilized ear spoon, such as in ordinary use, obtain a few drops, and inoculate at once one of the bouillon tubes with it. Without much exposure snip off a piece of the cortex with the sterilized scissors, and drop it into the second bouillon tube, and likewise obtain a small piece for one agar and one blood-serum tube. If there is any inflammatory exudate, dip the sterilized platinum needle into it, and then quickly inoculate a gelatine tube by thrusting the needle into the gelatine and withdrawing it. A stroke culture can be obtained by smearing the top of the agar in one of the tubes with the infected needle. Open the ventricles carefully with a sterilized knife or scissors, and with the platinum needle looped, or with the ear spoon, obtain a few drops for the inoculation of the third bouillon and one blood-serum tube. The other tube can be reserved for different portions of the cerebrum.

After the tubes have been inoculated, place two bouillon and two agar and two blood-serum tubes in the inoculating oven at 35° C. and watch developments. The other bouillon tube leave in the room at ordinary temperature, likewise with the gelatine and other agar and blood serum. If a distinct microbe is determined its nature must be studied by experiments on animals.

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the dressings, and on one occasion, while unobserved, he tottered down the ward. Beginning a few hours after this, there was almost complete suppression of urine, lasting forty-eight hours; but under treatment the secretion was re-established for a period of twenty-four hours, which was followed again by almost complete suppression, which lasted until death. Examination of the dressings showed pus and urine. A urinary fistula was evidently present. The man was irrational immediately after the operation, and this state gradually merged into one of moderate delirium, which on the seventh day amounted to actual mania, this lasting until death, which occurred ten days after the operation. The autopsy was made by Dr. H. W. Cattell. The right kidney lay in a bed of foul-smelling somewhat greenish pus resembling tubercular sputum. On section the renal substance was found infiltrated with pus, with great destruction of its upper portion. The kidney was of the large white variety, and contained a large, rather friable calculus, a perfect mould of the pelvis, infundibuli, and calices. The right ureter was thickened, but otherwise normal; the left presented a hydronephrosis, and on closer examination a stone the size of a lima bean, of medium size, was found plugging it at the junction of its inner and middle third. Examination of the brain and meninges was negative, with the exception of a slight haziness of the pia and some turbidity of the fluid in the lateral ventricles.

Cultures made from the meninges were negative; but from the fluid found in the lateral ventricles a pure culture of the bacillus pyocyaneus was obtained. Cultures from the pus in and about the kidney contained yeast and pus organisms, but not the bacillus obtained from the ventricular fluid.

The toxæmias of pregnancy and the puerperal state which result in mental disorder cannot be passed by in a discussion of this subject; indeed, thoroughly studied, they might afford the best foundation upon which to build our discussion. Davis,* who has recently contributed a brief but valuable clinical paper on the toxæmia of pregnancy, believes that the mode of production of the toxine or poisonous waste which threatens pregnant women is not clearly explained; that while the usual metabolic processes account for a portion of the material present, a certain number of the cases point strongly to an acute intoxication due to the products of bacteria. In one of his cases in which marked toxæmia was present the examination of the urine failed to reveal either casts, albumin, or marked deficiency in urea. Pronounced symptoms were present, as coronal and frontal headache and extreme restlessness and melancholia, greatly exceeding the usual timidity manifested by patients in the pregnant condition.

Tuke and Woodhead† believe that in puerperal insanity a consider-

* Davis: Amer. Journ. Med. Sci., Feb., 1894, vol. cvii., p. 147.

† Tuke and Woodhead: Tuke's Dict. Psych. Med., 1892, vol. ii., p. 911.

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able proportion of cases is due to toxic influences, for although a woman may become insane during her puerperal period, her case need not be referable primarily to childbirth. "However, the violent delirious mania which sometimes develops within fifteen days after delivery has all the aspect of toxic influences. The sudden inception, delirious character, rapid development, inflammatory complications, and tendency to death are eminently suggestive of septic origin. Such cases seldom present themselves later than a fortnight after childbirth (the period during which septic changes go on in the uterus), and more frequently they occur within ten days."

Olhausen,* in five and a quarter years, observed psychoses following eclampsia eleven times in two hundred cases. They occur early, seldom later than from two to four days, and are marked by constancy of hallucinations, rapid afebrile course, and generally favorable termination. They are doubtless intoxication psychoses, closely related to uræmic changes in the blood. He suggests the following classification: 1. Psychoses directly due to febrile puerperal processes (infectio-psychoses); 2. Idiopathic psychoses without bodily or febrile disease (lactation, acute anæmia); 3. Intoxication-psychoses, following eclampsia, or, exceptionally, uræmia without eclampsia.

While the mental disorders of the puerperium, like many other of its disastrous affections, are usually dependent upon the attendant and the surroundings of the puerperal woman, auto-infection has been claimed as an explanation of those cases which occur when every precaution has been taken. This question has been subjected to bacteriological investigation, especially by Williams,† who believes that the term auto-infection is not well chosen. Numerous careful bacteriological investigations have shown that the vaginal secretions of pregnant women are of two kinds—normal and pathological. The bacillus of the normal secretion is non-pathogenic, and, on the whole, is rather inimical to pus-producing organisms. In 10 per cent. of the cases which pathological vaginal secretion Döderlein‡ found the streptococcus pyogenes, which was demonstrated by inoculation experiments to be pathogenic in more than one-half of the cases, and these observations have been confirmed by Williams. The bacteria of puerperal infection, as summarized by Williams, are the streptococcus pyogenes, the staphylococcus aureus and albus, the gonococcus, and the colon bacillus. He holds that it is also quite probable that some of the

* Olhausen: Zeitschr. f. Geb., Bd. 21.

† Williams: Amer. Journ. Med. Sci., July, 1892, vol. xvi., p. 45.

‡ Döderlein: Das Scheidensekert und seine Bedentung für das Puerperalfieber. Leipzig, 1892.

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putrefactive organisms play an important part in what Matthew Duncan has designated as sapræmia.

While, so far as I have been able to learn, no satisfactory bacteriological examination has been made in a case of puerperal mania, Kaltenbach,* or rather Gerdes,† under the direction of Kaltenbach, made an exact and exhaustive bacteriological examination of the organs of a woman who died of puerperal convulsions; and the results obtained in this investigation would have equal significance in the explanation of fatal puerperal mania. Cultures were made from the lungs, kidneys, liver, and the aortic blood, and in all cases a growth developed which consisted entirely of a pure culture of a very short, thick bacillus. Its culture showed certain characteristic peculiarities. Injections into mice proved rapidly fatal, clonic convulsions occurring in about an hour, followed by death in stupor from nine to twenty hours. Relatively large doses of morphine, administered previous to the injection of the culture, prevented the occurrence of convulsions and saved the life of the animals. This drug, however, only prevents the effect of the germ, but does not inhibit multiplication in the body. The rat is quite refractory to this germ, needing large doses; rabbits, pigeons, and guinea-pigs are refractory. Gerdes suggests a very careful bacteriological examination of all fatal cases of puerperal eclampsia. In a later article, Gerdes‡ declares that the eclampsia bacillus is the sole cause of puerperal eclampsia, and is found in no other disease, and there can be no eclampsia without its presence; and that the infection proceeds from the uterus, probably from an endometritis existing prior to conception; 2. That convulsions due to other causes, occurring during labor, are to be strictly separated on the basis of the post-mortem appearances, from true puerperal eclampsia; 3. Eclampsia is a well-characterized disease, strictly limited anatomically; 4. The profound changes found in the organs of eclamptic patients post-mortem are not adequately explained by the demonstration of the presence of the specific germ in the body, but are probably due, directly or indirectly, to its toxines.

It must be noted, however, that several obstetricians and bacteriologists have reached different conclusions from Gerdes. Hoffmeister, Hagler, and Döderlein agree in declaring that they have not been able to convince themselves that the bacillus found by Gerdes is the cause of eclampsia. Hergott made cultures from the organs in seven cases of eclampsia without results. From the urine he isolated a bacillus

* Kaltenbach: Wien. med. Blaett., 1892, No. 21.

† Gerdes; Ibid, 1892.

‡ Gerdes; Deutsche med. Wochenschr., June 30, 1892, No. 26, p. 603.

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in five cases. This bacillus was not identical with that of Gerdes. It had some pathogenic action on pregnant rabbits, but Hergot believes that this is due to its action on the kidneys.*

Conclusions may be drawn as follows :

1. Specific infection must be included among the causes of mental symptoms and diseases which precede, accompany or follow febrile and infectious disorders.

2. Much negative evidence can be adduced in favor of acute delirium or acute mania being due to toxæmia; such evidence as is afforded by autopsies which reveal neither gross nor histological lesions; and in these cases the toxæmia probably overwhelms the patient before the production of meningitis or other disease.

3. Analogies with nervous affections, which are known or believed to be of microbic origin—such as multiple neuritis, myelitis, and chorea—favor the view that insanities with similar or related phenomena and lesions are also microbic in origin.

4. The evidence afforded by careful bacteriological investigation of cases of acute insanity is thus far meagre, and shows that various micro-organisms may induce the same or similar types of mental disease.

5. The mental disorders of pregnancy and the puerperal state are probably, in a considerable proportion of cases, toxæmic, without reference primarily to childbirth; but it cannot be regarded as proved that a bacillus of either eclampsia or puerperal mania is the sole cause of these affections.

* Wien. med. Woch, 1893, No. 22. Cited in Rev. Insanity and Nerv. Dis., December, 1893, vol. iv., No. 2.

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THE THERAPEUTICS OF INFECTIOUS PROCESSES IN THE NERVOUS SYSTEM.

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A consideration of the treatment of infectious nervous processes involves not only the treatment after infection has occurred but also its prevention. The latter is not the least important part of our subject, and to it we will first give our attention.

From much that we know we have reason to believe that the occurrence of infection is markedly influenced by the nervous system. Charrin and Ruffer, in experiments on animals, discovered that section of the sciatic nerve under certain conditions favored pyocyanic infection. Roger cut the sympathetic nerve in the neck, and saw erysipelas which he had inoculated into the ear of the same side disappear, while on the other side it continued to develop. In another instance, instead of the sympathetic this investigator cut the auriculo-temporal nerve, when the reverse result was observed, for the streptococcus developed more readily on the side of the section. Not only did he prove that the nerves influence the process, but that different sections produced different results. Startling as are these facts, they are, after all, in harmony with what we know of the control which the nervous system exercises over all of the functions of the economy. Not only does it dominate digestion, circulation, and nutrition in general, but also absorption. The vaso-motor nerves, through their influence on the size of the vessels and probably on the size of the stomata of the capillaries, are a powerful factor in determining the result. It is probable that the destructive inroads of germs are at times favored and at other times delayed or prevented by their action. Thus when the vessels are constricted and small, it is probable that invasion can take place with little or no interference on the part of the organism. On the other hand, if the vessels be in their normal condition or perhaps a little dilated, it is probable that the white blood-corpuscles pass out into the surrounding tissue and act as phagocytes upon the invading germs, and that in addition the liquid constituents of the blood exert an action both bactericidal and antitoxic.

If the rôle of the nervous system is so important, if the ingress of microbes is to so great an extent under nervous control, prophylaxis

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assumes an importance altogether peculiar. It becomes at once evident that our ability to resist infection depends largely upon the maintenance of a normal nervous tone—*i. e.*, nervous health. Indeed, we may here find a partial explanation of the difference of susceptibility displayed by persons exposed to infection under similar circumstances. That these thoughts are not mere speculative fancies is proven by such experiments as those of Charrin and Roger on the rôle which fatigue plays on the evolution of infection. These experimenters constructed an apparatus, on the principle of the squirrel-wheel, by means of which they brought about marked muscular fatigue in animals. They conclusively proved that this fatigue distinctly favored the development of infection. Thus, in one series of their experiments they used thirty-six rats; of these they reserved fifteen as control animals. Twenty-one they inoculated with the germs of charbon, and then by means of their apparatus caused them to walk for from two to eight hours daily for three days in succession. It was found that the resistance of the animals to inoculation varied according to age and size. In each series of experiments the investigators were careful to employ specimens of nearly equal size, taking care to preserve one, if any the smallest, as a control animal. All were inoculated into the skin of the flank. Three of the control animals withstood perfectly nine drops of the virus. The same dose was administered to eight others who were then subjected to the fatigue test. One of these resisted the inoculation fully, a second died at the end of four days, four at the end of three days, and two on the day after the inoculation. By increasing the dose, the experimenters were able to kill one of the control animals, but the latter did not die until at the end of five days. Further studies made by them yielded the same results, and in every instance, both by microscope and cultures, they verified the cause of death. The bacteria were always discovered in the blood. By their experiments they were also led to believe that fatigue favors infection from the intestinal tract. The conclusions to be derived from these researches are so clear as to be self-evident.

We have learned from the investigations of Hodge that nerve cells undergo certain changes in the course of their functional activity and that these changes can only be interpreted as those of fatigue. When analyzed they resolve themselves into simple loss of substance. That there may be in addition other changes less tangible—changes which are attended by the formation of products the result of retrograde metamorphosis—is also probable. However, the simple story of tissue waste teaches us a fact of great significance, for loss of substance being synonymous with fatigue, it must also be taken as synonymous

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with weakness, and, in virtue of the rôle that the nervous system seems to play in infection, this weakness must be construed as synonymous with a diminished power of resistance to the invasion of germs. The first problem, therefore, that presents itself is the prevention, if I may be permitted to so state it, of undue waste of nervous substance through undue or excessive fatigue. To repeat what is known to every physician, that long-continued overwork, with insufficient rest and food, favor the development of various diseases, may seem unnecessary here, but that we are now in a position to frame an intelligent hypothesis based upon the experiments of Hodge, Charrin and Roger, and others, is certainly interesting and important. The value or rather the absolute necessity of a proper proportion of sleep and a proper amount of food follow as unavoidable corollaries, but another element must also be considered. It not only suffices that the nerve cell should have rest and that sufficient pabulum for its regeneration should be at hand, but to repeat what is universally known, exercise both mental and physical within physiological limits is absolutely necessary. The solution, therefore, of the question of a proper prophylaxis is answered thus far by a proper proportion of food, rest, and exercise. The nervous health is, of course, influenced by numerous other factors concerned in general hygiene and special etiology; but these, I take it, do not directly concern us here. Simple as the conclusions with regard to general prophylaxis appear, their value must not be underestimated. To realize that overwork leads not only to nervous exhaustion, a fact with which the laity are as familiar as ourselves, but that it also opens the avenues of infection, is one which should be appreciated in its broadest significance.

The question next arises: "Are there any means of *special* prophylaxis against this or that infectious nervous disease?" Here, unfortunately, the answer which we can give is hesitating and clouded with uncertainty. At once the preventive inoculations of Pasteur, Behring, and Tizzoni and Cattani suggest themselves, but at the same instant doubts as to the advisability and applicability of the methods arise. Roughly speaking, the question of preventive inoculation for infectious nervous diseases must be answered, as we shall see further on, in the negative. However, special prophylaxis appeals to us from another quarter. Supposing that the general economy is already the subject of bacterial infection, are there no means by which the spread of the infection to the nervous system can be averted or the chances for such infection diminished? Is there anything that can be done in the various infectious fevers, in general pyæmia, in suppurative otitis media, in lung abscess, empyema, etc., to prevent nervous involvement? Are

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there any means by which the meningitis, brain abscess, and neuritis, that every now and then make their appearance in the course of these diseases, can be prevented or the chances of their occurrence lessened? With regard to abscess, the question may often be answered in the affirmative, but at times the involvement of membranes, centres, or nerves is to be explained either by the excessive virulence of the infection or by the special susceptibility of the nervous system to the poison in question. At times, too, secondary involvement of the nervous system is to be regarded as related to the sudden suppression of some local symptom, as a purulent discharge or perhaps an eruption.

Where the nervous system is threatened by the existence in other structures of foci of infection, abscesses, and suppurating cavities, such as purulent otitis, mastoid disease, purulent inflammation of the frontal sinuses or of the orbit, lung abscesses, empyema, etc., the indications as to prophylaxis are very clear. A suppurating cavity, be it large or be it small, constitutes a perpetual danger, and the importance of securing free evacuation and free drainage, wherever such a cavity is situated, need not here be dwelt upon.

Practically, we must admit, questions of general and special prophylaxis are presented to the physician far less frequently than questions of treatment after infection has actually occurred. Here the problem again resolves itself into general and into special methods. The general indications are of course clear. They are: first, to arrest or limit the infective process; and, secondly, to bring about a rapid elimination of morbid products, toxines, etc., or the evacuation of tissue detritus, such as we find in purulent inflammation, abscesses, and the like. The first indication, namely, to arrest the infective process, is easier proposed than carried out. Unhappily we are not yet possessed of a germicide which will enable us to bring about the death of germs within the nervous system without injury to the tissues. Such a substance would be one that would act upon microbes as quinine appears to act upon the organisms of Laveran. It is in this field that chemistry and the biological laboratory may some day yield us a great discovery.

In the meantime Pasteur, Koch, Behring, Kitassato, Tizzoni and Cattani and others have attempted the solution of the problem on an entirely different basis. The familiar fact that the growth and multiplication of the torula is arrested by the very alcohol to which it gives rise, seems first to have suggested the attempt to combat the infectious microbes by the toxines which they themselves produce. However, with the possible exceptions of tuberculosis and leprosy, the symptoms produced by infectious micro-organisms appear to be due not so much to the germs as to these very toxines. Notwithstanding, these attempts

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have led to very interesting and in some instances very valuable results, and these we will briefly review.

As regards tuberculosis the story of the hopes and failures of Koch's lymph is too recent to bear repetition. In tuberculosis of the nervous system the lymph is shown not only to be useless but even dangerous. This is the experience of Brehm and others in tuberculous meningitis and even if it be given for tuberculosis of other structures, tuberculous meningitis may arise during its use. Caspersohn, for instance, observed such meningitis develop while he was treating his patient for tuberculous fistula and abscess of the right thigh and hip.

In the treatment of leprosy by the lymph, failure appears to have been almost equally marked. The writer has collected in all sixty-four cases treated by Koch's lymph in 1891. Not a single cure was reported among them all. Poupinel noted considerable improvement in two cases out of five. Kaliendero and Babes noted amelioration and improvement of general condition in eleven. Schwartz noted improvement in a number out of twenty-two cases, but did not follow them up. Goldschmidt noted improvement in one out of ten. It appears that even these general statements of amelioration and improvement must be taken with considerable allowance. Danielssen, after having treated fourteen cases, comes to the following conclusions: First, that there is a local and constitutional reaction which comes on from four hours to three days after the injection of the lymph. The local reaction occurs later. Second, that the reaction has no favorable influence upon the leprous trouble, but on the contrary it aggravates the latter. Third, that the tuberculin does not destroy the lepra bacillus, but that it fertilizes the soil for the latter, the baccilli increasing in activity and destructive qualities more than if the patient had received no treatment. Finally, he concludes that the method can produce a kind of immunity when the tuberculin is used for a long time, but that the method does not arrest the progress of the disease, for new symptoms may develop under its use. Whether modifications of the lymph or such methods of preparation will yet be devised as will eliminate all elements of danger, and whether it will finally be successful, can of course only be conjectured.

When we turn our attention to Pasteur's efforts with hydrophobia the story is decidedly more encouraging. It is obviously out of place here, nor would time permit us to go into a detailed discussion of Pasteur's methods, nor to consider in detail the claims and criticisms of his practical results. However, a brief review of his methods is necessary for our purpose. As is well known, Pasteur found that if the spinal cord of rabbits inoculated with rabies are kept in a room

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with a constant temperature of 20° C. (68° F.), they gradually lose their virulence with each day's action of the dry atmosphere and the surrounding temperature until, after the expiration of fourteen days, a material remains which produces no perceptible action upon rabbits or dogs when introduced into the brain. It does, however, protect the animal against future inoculations with most active forms of the virus. It appears that the process of drying destroys the germ (whatever that may be), but leaves the toxine produced by the germ practically unchanged. Pasteur obtained his virus by inoculating rabbits beneath the dura mater with the spinal cord of the rabid dog. Then this rabbit having developed rabies, its spinal cord is inoculated into a second rabbit, and this process is repeated until finally it is found that the virulence has increased to such an extent that a small particle of the cord injected into a rabbit will kill in seven days. The spinal cord of the last animal is then attenuated in dry air as described above. A small fragment of this material is then diluted in sterilized bouillon. These injections are repeated with material obtained from cords of greater virulence until finally a very virulent material is injected, when the animal is found to be immune. Pasteur applied his method to the treatment of patients suffering from the bites of rabid animals, by beginning with material obtained from cords that had been dried for a period of fourteen days and by steadily following the initial injection with material obtained from cords of less attenuation until at last the cord of one day's drying was used. The injections were made under the skin. The theory of Pasteur is that a gradual accommodation of the organism to the virus occurs.

As is well known, Pasteur's claims were confirmed by the English Committee of Investigation. This committee reported that the experiments of Mr. Horsley entirely confirmed Pasteur's discovery of a method by which animals may be entirely protected form the infection of rabies; and further, that the personal investigation of Pasteur's cases by members of the committee was, so far as it went, entirely satisfactory, and convinced them of the perfect accuracy of his records. Among those who have also given attention to the question of hydrophobia experimentally must be mentioned Tizzoni and Cattani. These investigators experimented with the vaccine extracted from the central nervous system of rabbits dying from fixed virus, though even a vaccine prepared from the first generation after inoculation gave positive results. Ten cubic centimetres of their liquid contains dissolved the active principle of about one gramme of rabic nervous system. This solution failed absolutely to produce rabies by experimentation, and was entirely aseptic. With this liquid they succeeded in making

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twelve out of fourteen rabbits entirely immune. In the two in which preventive inoculation had been unsuccessful the quantity administered seemed to have been insufficient. Tizzoni and Cattani concluded that one inoculation was sufficient, and that even on the following day the animal may be inoculated with rabic virus without developing the disease. They further proved that their vaccine was efficacious even when administered during the incubation period of rabies, though a larger dose is in such a case necessary. They concluded, however, that in the case of rabbits the treatment must not begin later than four days after the infection.

More recently Tizzoni and Cattani made the interesting discovery that in sheep and dogs which had been injected under the dura mater with rabic nervous system the serum of the blood acquired an anti-rabic value. This anti-rabic quality increases in intensity until the twenty-fifth day after vaccination—that is, sub-dural inoculation—and then begins to decrease. This is the time when the serum should be removed from the animal. The serum, they claim, produces no unpleasant effects whatever and is absolutely lacking in virulence. These interesting and original observations are in harmony with those made previously by them regarding the immunizing and therapeutic effects of the serum of rabbits who had been rendered immune by vaccination with the rabic virus. The scientific importance and wide significance of these discoveries cannot be overestimated.

The story of the extensive application of Pasteur's methods, his extravagant claims as to results, together with the active opposition they aroused in certain quarters, is still too recent to bear repetition. In Paris itself his claims, as is well known, have been actively combated by Lutaud, who showed that in France over a period of twenty-three years (1850 to 1872) there were but 685 deaths from rabies—that is, about 30 a year. Pasteur claims that during the five years 1881 to 1885, there have been 60 deaths in the hospitals of Paris. This is disputed by Lutaud, who cites cases with the hospitals at which they were treated, showing only 26 deaths. Again, Pasteur claims that the annual mortality for seven years up to 1870 is 76; Lutaud proves it to be 47.7. Vulpian states, in Pasteur's name, that the number of persons bitten by rabid dogs in one year was 100 and the number of deaths 16, and that these occurred in persons not treated by the Pasteur method. He then follows this statement by saying that out of the 1726 cases treated only 12 died in place of the 276 who would otherwise have died without treatment. Lutaud called attention to the remarkable increase of the number of cases of rabies implied by the statistics. Pasteur says that of 1929 Frenchmen treated 18

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died up to December 31, 1886; all told there were 2682 cases of hydrophobia. Lutaud then shows that the cases of death in the Pasteur Institute during the year 1886 numbered 22, and further that there were also 17 deaths without treatment, making a total of 39 deaths for the year 1886, which is an excess of the mean annual mortality of previous years, which is 30. From 1886 to 1890 there were 90 deaths after Pasteur's treatment had been employed. The cases, names, addresses, etc., are recorded. If we add to this the number of deaths of those not treated, we have the figures 154, a mean of 38 deaths a year.

Among the opponents of Pasteur's methods must also be mentioned Dulles, whose criticisms have been exceptionally severe, as the following citation from a paper read before the recent meeting of the State Medical Society of Pennsylvania will show:

"The number of cases of hydrophobia that occur in this country is happily small. It would doubtless be smaller still but for the exploitation of the Pasteur Institute conducted by Gibier in New York, and of its feeble imitator conducted by Lagorio in Chicago. These institutions and the newspapers that, in times past, have published sensational accounts of cases of so-called hydrophobia, have in a mild way reproduced some of the conditions which make France the hot-bed of hydrophobia, as well as of hystero-epilepsy. But the psychological make-up of Americans is less favorable to the development of the germs of hydrophobia or those of hystero-epilepsy than is that of the French, and consequently there is less of either here than there is in France. There the history of the last six years differs but little from that which I described to you in my last report. As then, so now, the number of deaths in France is greater than it was before Pasteur, just ten years ago (in May, 1884), boasted to a newspaper reporter: 'Whoever gets bitten by a mad dog has only to submit to my three little inoculations, and he need not have the slightest fear of hydrophobia.' The year before he made that boast there were 4 deaths from hydrophobia in Paris (the department of the Seine); the year after, when he had practiced his preventive method for six months, the deaths from hydrophobia leaped at once from 4 to 22. In 1886 the number fell to 3 again in Paris; but I have a list of 23 persons that died after treatment by Pasteur himself in that year. In 1887 the deaths in Paris rose to 9, in 1888 to 19. These oscillations indicate that Pasteur's method is no more preventive of hydrophobia than is the method which he declared, in 1884, would eradicate rabies in dogs. On the contrary, Pasteur's method has undoubtedly increased the number of deaths from hydrophobia. I have indicated what has taken place in France and can assure you that there has been no diminution of the number of deaths from hydrophobia in any part of the world since Pasteur's infallible cures were inaugurated; and at the same time there has been added to these a large number of deaths due to inoculation with virus of what ought to be called 'Pasteur's

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disease.' Just how many these have been, no man can say. The statistics in regard to these deaths are confusing. Those from friendly sources contain remarkable discrepancies. Pasteur's own statistics, published in the *Annales de l' Institut Pasteur* for March of this year, admit 72 deaths in seven and one-half years after treatment in Paris. My own statistics show a much larger number, while I find that Dujardin-Beaumetz, an enthusiastic supporter of Pasteur, reported to the Académie de Médecine on June 21, 1892, 98 cases in only six years, which is just 26 cases more than Pasteur himself reported for eight years. One way in which such curious figures appear may be seen when we examine Pasteur's detailed report for 1893, when we find that 10 actual deaths are set down as 4, because 2 of the 10 unfortunates succumbed in less than fifteen days, 3 developed their fatal disease while receiving the inoculations, and only one did not stay to have the treatment completed. The same manipulation may be found in the reports for other years."

In the limited time at our disposal a detailed review of the evidence *pro* and *con* of Pasteurism and hydrophobia is impossible. However, it is vain to deny the truth of Pasteur's experimental researches upon animals, while the evidence as regards human beings is in *specific* instances strong and convincing. It goes without saying that of the number of dogs who inflict bites upon human beings a percentage only are rabid, and finally, that of the number of persons bitten by rabid dogs only a portion become infected with the disease. The publication of Pasteur's methods, the wide distribution among the laity of information concerning his treatment, and the well-known predisposition of the French to hysteria, have increased enormously the number of persons who have directed medical attention to bites received by them, and in this way is doubtless to be explained the enormous increase in the number of cases.

However, in a disease the mortality of which untreated is practically 100 per cent., any method of treatment that yields even a ray of hope deserves trial. Inasmuch as the period of incubation is generally prolonged, there is certainly time for giving a patient the benefit of Pasteur's method. It would seem that the rational course in a given case would be to remove the spinal cord of the supposed rabid dog, and to perform with it test inoculations upon rabbits, and having determined the actual existence of rabies by this means, to rapidly prepare attenuated material. The evidence shows that the risk involved by the injection of such material is exceedingly small. Let us hope that the discovery of Tizzoni and Cattani of the anti-rabic value of the serum of dogs and sheep upon which sub-dural inoculations have been practised, will lead to a simpler and more direct method.

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When we turn our attention to the subject of tetanus we find the existing state of investigation to be more interesting still. In 1890, Kitasato attempted to make animals immune to this disease. His attempts by means of repeated inoculations of attenuated virus, failed to yield a satisfactory result. However, by applying the method which Behring had devised in experiments with diphtheria, he finally succeeded. He first injected a middle-sized rabbit with 0.3 of a cubic centimetre of the filtrate of tetanus culture subcutaneously. Immediately afterward he injected into the same area 3 cubic centimetres of a 1 per cent. solution of iodine trichloride. In twenty-four hours the same dose of iodine trichloride was again administered. In forty-eight hours some symptoms of tetanus infection appeared. The dose of iodine trichloride was again repeated, and twice thereafter; in all, five doses were administered before tetanic symptoms finally disappeared. The animal was entirely well in ten days. On the fourteenth, eighteenth, and twenty-fifth days large doses, 2 to 3 cubic centimetres, of tetanus culture were administered with negative results. Two control animals which received but a moderate dose of the culture, died in a few days. This method was successful in producing immunity in 40 per cent. of the animals experimented upon, and further, this immunity persists for two months. Experiments were further made to render other animals immune with the blood of immunized rabbits. Kitasato injected liquid blood, before coagulation, into the abdominal cavity of mice, who, with control animals, were subsequently inoculated with tetanus bacilli, and in such a dose that the control animals died in thirty-six hours of tetanus; the others remained immune. The serum also revealed therapeutic properties. An animal could be infected with tetanus, and after symptoms had appeared, successful results were obtained by injecting the serum into the abdominal cavity. He further proved that when the filtrate of tetanus culture is mixed with the serum of the tetanus-immune rabbit, it fails to kill the mice, while control animals perish. Behring also experimented extensively in the same field, and found that iodine trichloride added to his carbolized tetanus culture in increasing doses, made the culture less and less virulent, and finally innocuous. Rabbits which survived injections of this mixture were able to stand more than the minimal dose necessary to kill control animals, and thus attained a certain degree of immunity which, through further inoculation with culture-fluid, became greater and greater. Behring also, and Schultz later, demonstrated the possibility of thus immunizing horses and sheep.

More interesting than all, however, are the combined researches of Brieger, Kitasato, and Wasserman. To begin, they noted that when

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they attempted to grow the tetanus bacillus in a watery infusion of thymus gland, that they were only occasionally successful. They then made the remarkable observation that the tetanus bacillus, though it grew, failed utterly to develop spores. Even when they submitted the cultures to the incubator for as long a period as fourteen days, the result did not vary. However, when these sporeless bacilli were transferred to other culture media they again developed spores. That is, the tetanus bacillus simply loses the power of spore-bearing while in the thymus infusion. These investigators then followed up this discovery by inoculating animals with cultures made in thymus infusion, with the result that they were able to produce immunity in all of the animals experimented upon. While the trichloride method yielded 40 per cent. of immunized animals with the immunity persisting for two months, the thymus method yielded immunity in 100 per cent., and at the end of four months this immunity was as marked as ever.

Behring, Frank, Kitasato, Tizzoni and Cattani, were successful in conferring immunity upon other animals by the injection of immunized serum. They all demonstrated also that this serum possessed in addition curative properties; that is, animals in which tetanus had been produced by inoculation, and which were afterward treated by injections of immunized serum, recovered, while control animals perished. Further, they all found that the dose required to bring about a cure was far greater than that necessary to bring about immunity. The practical application of these striking and brilliant results in the cure of tetanus in man, possesses an interest that is altogether peculiar. In a disease, the mortality of which is variously estimated at from 75 to 90 per cent., being second only to hydrophobia, the introduction of a really successful plan of treatment would rank as one of the greatest achievements of modern medicine.

Inasmuch as the quantity of serum which could be derived from mice and rabbits was obviously too small to be available in man, Behring experimented upon horses and sheep. Even under these circumstances it seemed at first as though the quantity of serum required would be too large to be practicable. However, it was found that the serum of the immunized animals increased in strength with time, and Behring finally succeeded in immunizing the serum of horses until it attained an immunizing value of 1 to 1,000,000 body-weight. Of this serum one gramme will be sufficient to immunize 1000 grammes body-weight each, against a minimal fatal dose of tetanus culture.

According to Behring and Frank, and also Tizzoni and Cattani, the healing value of the serum is 1000 to 2000 times less than its immu-

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nizing value. Therefore a serum which has an immunizing value of 1 to 1,000,000 would have a curative value only 1 to 1000. That is, for every kilogramme of body-weight one gramme of serum should be used. Again, in cases in which the disease has already advanced, or in which the infection has already produced severe symptoms, a still larger quantity must be employed. It was a serum of this strength that was applied by Rotter in the treatment of the case of tetanus in man. The patient was a male, twenty-five years of age, who had developed tetanus after a wound of the hand. The first injection was not made until seven days after the appearance of trismus. 66 grammes of immunized horse-serum, containing 0.5 per cent. of carbolic acid, were injected into four places beneath the skin of the back. On the day following his condition remained unchanged. A second injection of 50 grammes of the same serum was now administered. The seat of the injection was the left pectoral region, the needle being turned in different directions. The pain caused was slight. On the following day the patient expressed himself as feeling decidedly better. Trismus and rigidity of the back of the neck and of the legs were somewhat lessened, though not markedly so. Twitching ceased. 45 grammes of serum were not administered into the left Mohrenheim fossa. No reaction was noticed at the punctures or area of the injections. Distinct improvement was noticed on the following day in all of the symptoms. The fourth and fifth injections were then administered on successive days. The symptoms now steadily improved, though slowly. On the seventeenth day, after the last injection, the patient was dismissed from the hospital, with a slight rigidity of the left forearm, which five days later entirely disappeared.

Another interesting case reported is that of Moritz. Behring had succeeded in bringing up the immunity of the serum of a horse to so high a value that it was estimated to have an equivalence of 1 to 10,000,000—that is, 1 cubic centimetre would be equivalent to rendering immune 500,000 mice of 20 grammes each, or 143 human beings of 70 kilogrammes each, against a minimal fatal dose of the tetanus poisoning. These figures seem almost incredible.

It was a serum of such strength as this which Moritz used in a boy twelve years of age. Unlike Rotter's case, the attack of tetanus did not follow a trauma. One week after the appearance of trismus 20 c.c. of an immunized serum were injected into the inner side of the left thigh, while 10 c.c. were injected into the corresponding portion of the right thigh, and 20 c.c. into Mohrenheim's fossa. The next day 30 c.c. additional were injected, while on the day following, 15 c.c. more were given, in all 95 c.c. By this time such decided im-

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provement had set in that the injections were discontinued. Three weeks after the beginning of the treatment the boy was able to walk, and some ten days later resumed his school.

In Italy this method of treatment has been employed in quite a number of cases, as many as twelve having been thus far reported. Tizzoni and Cattani, from their experiments, came to the conclusion that for practical purposes in man, when the strength of the serum has reached 1 to 1,000,000 it is most fit for use, and that the value of the serum depends only on the amount of the antitoxine it contains and not upon its origin—that is, not upon the particular animal from which it is derived. It would be unnecessary, therefore, they found out, in a case of necessity, to wait until increasing injections had afforded a maximum intensity to the serum, but to at once ascertain the strength of the latter and then to administer it in a proportionate dose.

These investigators also showed that the alcoholic precipitate derived from the serum is of the same value as the serum itself; though Behring maintains that the antitoxine does not exist in Tizzoni and Cattani's dry powder in its purity. Behring also estimates the value of Tizzoni's antitine as but 1 to 10,000. It would, therefore, have a far lower value than that of Behring's serum. Tizzoni estimates, however, that of his serum, 70 c.c. would be the dose for a case of moderate severity in man in the beginning. At a later stage, 210 c.c. would be required. This would equal of the alcoholic precipitate 5 to 6 centigrammes in the first instance and 10 to 12 grammes in the latter.

The writer has collected in all some thirty-four cases of tetanus of various origins, treated either by the powder of Tizzoni or by the serum. Of these, twenty were successful, certainly a large proportion. However, it is not improbable that in some of these cases the recovery was brought about not only by the antitoxine employed, but also by other measures, *e. g.*, amputation or early active treatment of the wound. Further, in the majority of the Italian cases the period of incubation was relatively long and the cases, as a whole, seemed of a mild type. Certainly they were neither very acute nor very severe. Rotter in his criticism of the first eight Italian cases goes so far, indeed, as to maintain that not one of them taken alone can be considered as proving the curative properties of Tizzoni's antitoxine. Finally, it is to be feared that in the long list of successful cases reported, the unsuccessful ones have been most unwisely omitted. If this suspicion be incorrect, the twelve Italian cases, notwithstanding their mild character, constitute a strong argument in favor of Tizzoni's antitoxine. A number of suc-

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cessful as well as unsuccessful cases have also been reported in Germany and France. Thus Roux and Vailliard were successful in two cases, unsuccessful in five; Renon was unsuccessful in two; Escherich successful in one, unsuccessful in three; Range lost two; Baginsky and Kitasato lost one; Schwartz another. The unsuccessful cases appear to be as much open to criticism as the successful ones. In some the treatment was begun too late, in others an insufficient dose seems to been administered; in others still, the serum appears to have been insufficient in intensity. In the successful cases the symptoms are never abruptly arrested but disappear slowly, the case often running a prolonged course.

Various speculations have been advanced regarding the method of action of the antitoxine. Tizzoni and Cattani, for instance, believe that the treatment does not so much affect the symptoms that are already present as that it limits the spread of the disease by immunizing or protecting the portions of the nervous system not already affected; that for this reason it has little or no influence on the symptoms already developed. Thus Tizzoni and Cattani proved that when the injections of tetanus culture exceeded a certain amount, or the symptoms had widely progressed, that the serum was insufficient to cure. It is, however, extremely probable that the antitoxine possesses the property of directly destroying the tetanus poison. This is the position assumed by Behring. It is difficult to reconcile the theory of immunity with any other hypothesis. A statement of practical, even if of negative importance should be added, and that is that there exists no contra-indication to the use of the serum or of the antitoxine. Its employment appears to be in no way injurious. While the evidence regarding its actual therapeutic value is unfortunately inconclusive, it is still to be hoped that further trial will yield a decisive answer. The promise held out is so great that it not only justifies but absolutely demands this further trial. We learn from Behring that the serum can very readily be preserved by the addition of 0.5 per cent. of carbolic acid, while Tizzoni's results show that the form of a dry powder it retains its virtues for a long time, and it is not improbable that the methods of preservation will yet be so far perfected that, like other remedies, they can be preserved for long periods and use at will.

The methods indicated by the researches of Pasteur, Koch, Behring, and Tizzoni are not apparently to be limited to the diseases we have thus far discussed, for from Behring we receive the hope that they may also be applied to the treatment of diseases due to streptococcus infection. When we contemplate these methods, based as they are upon scientific investigation and deduction, even when we examine most

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critically the practical results, we realize that the hope they offer of a final conquest of these terrible diseases is nothing less than sublime.

The various other means at our disposal for combating infectious nervous processes resolve themselves into general remedies, drugs, and surgical procedures. It would obviously be out of place before an audience of this character to review well-established and well-recognized principles of treatment pursued in the various infectious nervous diseases, and I deem it my province here to touch simply here and there upon that which may be new. Unfortunately, little that is new can be said under any of these heads. Cold has been applied with varying success, as is well known, in meningitis, both cerebral and spinal, though one of the later writers (Brehmer) saw ill effects follow its use in children. Heat also has a comparatively limited application. Horsley proved experimentally that in hydrophobia it is worse than useless. Baths are rarely employed in infectious nervous diseases. It is not improbable that they would prove of benefit. From the remarkable results of the Brand method in typhoid fever the question arises whether the therapeutic result is due only to the falling temperature, or perhaps to some other action of the water, and it has occurred to me that in such affections as tetanus and cerebro-spinal meningitis repeated and prolonged immersion in water of suitable temperature would be a justifiable expedient. Whether by this means the elimination of toxines would be favored, is a matter that can at present only be conjectured. It is interesting in this connection to note that Bennett of Texas reports a successful case of tetanus cured by frequent immersion in hot baths of from thirty to sixty minutes' duration.

When we turn our thoughts to treatment by drugs we meet little that is either encouraging or gratifying. For hydrophobia the treatment by drugs is equivalent to zero, and for tuberculosis of nervous structures their value cannot be said to be any greater. As regards tetanus, to the various drugs, bromide, chloral, morphine, and the other sedatives that are so familiar to us, a long list, self-condemnatory by very reason of its length, is constantly being added day by day. They include such drugs as carbolic acid, eserine, strychnine, salicin, pilocarpine, aconite, antipyrin, and even the poison of the rattlesnake.

As regards leprosy, benefit is said to have followed the use of gurjun and oil of chaulmoogra used internally and externally. Chlorate of potash in almost toxic doses is said also to have been followed by good results. Europhen yielded benefit in one case and proved useless in four others. As regards chorea, which, though not demonstrably in-

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fectious, is probably so, quinine has recently been advanced by H. C. Wood as possessing unusual value. Extensive trial has, however, shown that it remains secondary in value to arsenic. Antipyrin has also its advocates, and especially is this true of exalgin, which Dana thinks has a specific influence in Sydenham's chorea.

On the whole, the literature affords little that is new on the treatment of infectious nervous diseases by drugs.

Surgical procedures enable us every now and then to accomplish definite and often brilliant results. This is especially true with regard to the evacuation of pus in positions that were formerly deemed inaccessible, *e. g.*, brain abscess, which is no longer the frightful and hopeless disease of former times. Indeed, it now constitutes, to use Macewen's words, "one of the most hopeful of all cerebral affections." It is earnestly to be wished that some surgeon will yet devise a means for the successful treatment of infectious diseases of the various membranes. Certainly, what we know of their hopeless nature, purulent inflammations would justify even extraordinary procedures. In keeping with these thoughts, C. A. Morton tapped the arachnoid space in four cases of tuberculous meningitis and noted slight improvement in two of them, and Wallace, Ord, and Waterhouse trephined through the cerebellar fossa of a child of five years. A small quantity of fluid escaped, a drainage tube was inserted, and the child did well. In tetanus also surgical interference has proved of great value in regard, of course, to the thorough antiseptic treatment of the wound, to excision, and to amputation.

In closing my part of the discussion—a part which I fear has been anything but satisfactory—I desire to make a suggestion in reference to the treatment of tetanus which may be of value. It will be remembered that Brieger, Kitasato and Wasserman discovered that the tetanus bacillus, though it grew in thymus infusion, did not develop spores, and that animals inoculated with such cultures were made highly immune to the cultures of tetanus grown in other media. The thought suggested itself to me that if the thymus juice possesses such remarkable properties, it should be tested with a view to a possible therapeutic effect. Its administration in a case of tetanus beneath the skin could certainly do no harm and might do good, and it might also be administered by the mouth, like the thyroid gland in myxœdema. If successful, it would prove far more valuable than the antitoxine, because so readily procurable.

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THE PRESIDENT'S ADDRESS.

BY ALFRED L. LOOMIS, M.D., LL.D.

THE INFLUENCE OF ANIMAL EXPERIMENTATION ON MEDICAL SCIENCE.

GENTLEMEN:—This third meeting of the Congress of American Physicians and Surgeons is a most memorable occasion, since it has established the success of an undertaking, which aimed to unite in one representative body groups of acknowledged experts in all departments of Medicine and Surgery. It is, however, no longer an experiment, for the record of work done places it high in the list of scientific associations. Its broadening influences upon American Medicine and Surgery have already been felt and acknowledged. The vast extent of modern medical research, the various objects of separate interest which it includes, and the limitations of human intellect, have made it necessary that there should be groups of workers in many different departments.

It is clear that one mind cannot advance medical knowledge except by an infinitesimal degree. In ages when strong minds were few and intercourse limited, men must needs have been more than human to have accomplished much more than their masters did. For nearly a thousand years the history of medicine may be traced by a few names. Schools were founded on men, not on principles, until loyalty to one's master, the founder of a school, became stronger than truth. Even as late as the sixteenth century, Galen, Hippocrates and the Arabic authorities were the teachers of medicine throughout the civilized world. From the crumbling ruins of abstruse theories and the wrecks of individual systems has come a scientific scepticism, which is to-day the most striking characteristic of medical thought,—a scepticism which doubts not for the sake of doubting, but which demands proofs and counter-proofs, which scans facts, not men, and which learns to recognize truth from whatever source it comes. It is this, which is making modern medicine truly scientific and giving to modern investigation an individuality which thinks and decides for itself. It is this spirit, this love of truth, dominating such gatherings as this, which, during the past two decades, has been shaping medical

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thought and investigation. It is this keeping in close touch with one another's work that is giving to modern medicine its freshness, special activity in investigation and rapid growth, which is inspiring medical workers with a community of thought and action, and which is bearing fruit of the greatest promise. On such an occasion as this, in the presence of so many trained and skilled workers, it seems fitting that I should direct the current of thought to the lines of investigation which have made the discoveries of the last quarter of the nineteenth century possible.

From the beginning of history until the present century, medicine has been either absolutely denied a place among the Sciences or else branded as inexact, empyrical and laggard in its development and progress. Although dealing, as it does, with the most complex problems of human existence, where, as in no other science, every law of nature is controlled and modified by that unknown force we call *vitality*, Medicine has nevertheless, from the very first, been forced to meet the demand for complete knowledge. To it alone, the answer "we do not yet know all" has been denied.

No greater misconception has ever gained footing in the public mind than the belief in disease as an entity,—an evil spirit to be exorcised or driven out by drugs. The superficial observer recognizes only results and gross phenomena; he is content with knowing the end, never asking for causes. For him motion and quiescence as shown by his senses are the ultimatum; factors and forces have no place in his mental processes. Yet these are precisely what science seeks to define, and until he has made the first analysis of terms, established absolute variations of quantity and quality, and determined the fixed ratio of forces, the scientific worker is not content.

In determining then the influence of any one factor in the development of medical science, results cannot be measured by the perfection of the whole, but must be estimated solely by the degree of advance toward the completed investigations.

The specific problems with which medical science deals are thus seen to be questions of the relative influence of multiple forces on the production of given results. When Galvani recognized electric force in the twitching muscles of his dismembered frogs and Volta was led thereby to the development of apparatus for the continuous production of this mysterious agency, there was no hint whatever of the far-reaching influence of electricity in modern medicine. Yet history shows that one discovery was the direct result of the other and that every electrical device for the relief of disease has its origin in those quivering batrachian limbs.

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Only the deepest ignorance can fail to recognize that the forces concerned in the simplest changes of inorganic nature are so numerous, and their relations so complex, that they defy recognition under uncontrolled conditions, while in the organic world the task is even more hopeless. Experimentation, therefore, in which one or more of the involved forces can be controlled or predetermined and eliminated, becomes an absolute necessity in all scientific investigation. However clear the mental analysis, however accurate the logical demonstration from cause to effect, it is possible by experiment alone, under controlled conditions, to prove that no involved force has been overlooked; that the premises were true and the conclusions therefore ultimate. The truly scientific investigator is an analyist and purist, who seeks to establish the values of single, in place of combined, forces. His results are therefore primarily isolated facts, and their value not immediately evident, often indeed their relations are so remote, and their values so contingent upon yet other undetermined truths that they gain but scanty recognition; if perchance they are not totally ignored, and finally forgotten. Meanwhile the brazen dicta of some mere observer, who only sees most superficial relations and blindly accepts an assumed possibility as demonstrated fact, gain unhesitating credence. Yet the scientific experimentator alone, adds to our store of knowledge and power for good. He seeks for truth and truth alone. However isolated or unrelated his results may seem, he sees in each a potential value. The smallest discovery thus not only has its own pecular merit, but points the way to other hidden truths, although it may often be difficult to connect the several links in the long chain of progressive knowledge.

The world might still be standing in dumb awe and barbaric fear of Jove's thunder-bolts, had not Franklin's kite decoyed the blinding force and locked it in his Leyden jar. Thus bound and under predetermined conditions of action, it was brought within the power of scientific investigation.

Is it not strange that Medicine should be denied the right to follow those imperative methods of scientific research which are so unquestioningly accorded to every other science?

It has been assumed that the medical investigator finds ample opportunity for experimentation in studying disease under the ordinary conditions of human life.

Recognition, however, of the fact that experiments are never isolated, but in continuous and consequent series; that unknown quantities are determined, as in solving algebraic equations, by successive eliminations, and that ultimate values are obtained only from experi-

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ments involving but a single unknown term, at once indicates how uncertain, and hence valueless, conclusions drawn only from clinical experience must be. If further proof be desired it is found in the well-known uncertainty and variability of disease processes, and the associated systemic reaction. Scientific experimentation thus demands conditions under which the largest possible number of the involved factors can be controlled or known. For medical science these conditions can only be found in a healthy organism. This science must therefore either stand still, or investigate the mysteries of life, where life holds its myriad forces in perfect harmony. Thus, it seeks not primarly to discover cures for disease, but rather to separate the multiple factors of disease and to fix the relations of such factors to the forces under our control, by which they may be modified. It is not a little surprising that, with an appreciation of the necessity for experimentation, men should for so long have preferred to be its subjects, and that even to-day so many refuse to yield the place to animals. For example, in wide-spread epidemics we note the effects of an infection on perhaps half a million of human beings, with a great sacrifice of human life. On the other hand we study in laboratories the cause of the epidemics, with comparatively small sacrifice of animal life.

In entering upon the consideration of this subject I fearlessly lay down this proposition, confident that it states your unanimous verdict as representatives of medical science on this continent. That every distinct advance, every established principle, and every universally accepted law of medical science has been in the past and will be in the future the indirect, if not direct, result of animal experimentation. I ask you to review with me some of the more obvious and conclusive proofs of this proposition.

The imperative lines of our investigation may be broadly classed under four heads:

1. *Experiments to determine the functions and normal relations of the organs composing the physical economy.*

2. *The causes of those perversions of function present in the condition designated disease.*

3. *The nature of morbid processes and the relations of their causes to the consequent systemic reactions.*

4. *The protective and curative influence upon these processes of agencies under our control.*

In this review I shall follow an historical order, and present in an Appendix detailed accounts of all experiments to which reference is made.

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It is not too much to claim that during the latter half of the present century the results obtained from experiments on animals have done more than all the observations of the preceding centuries to raise medicine from conditions of vagueness to conditions of exactness. From the time of Aristotle (1), who proved that the blood, brain and spinal marrow in animals have no sensation, down to the present day, animal experimentation has been practised by all investigators, who have gained any definite knowledge of the more important phenomena of animal life.

Galen (2), however, must always be regarded as the pioneer in this line of investigation. He may be said to have laid the foundation stone of medical science. By his experiments on living animals he showed that arteries contain blood, that the lungs passively follow the movements of the chest, and that the diaphragm, although the most important, is not the only muscle of respiration. Furthermore, by section of the spinal cord and the recurrent laryngeal nerve, he demonstrated the nervous control of the voice and explained the mechanics of respiration. By various methods he greatly advanced the knowledge of the structure and functions of the alimentary canal, demonstrated the movements of the stomach and the peristalsis of the intestines and laid the foundation of our present extensive knowledge of the functions of the brain and spinal cord. The results of his experimental work are "as conclusive now as when he first made them, and retain to-day their full value;" in fact they are the only part of his vast labor which has stood the test of modern investigation. The knowledge of the circulation and respiration which was gained by his experiments on animals in the second century was the foundation and a necessary preliminary to Harvey's complete discovery of the circulation in the seventeenth century. His experimental labor was the only work that has survived the fluctuating medical systems of the Dark Ages, with perhaps two exceptions, viz.: First, Vesalius (3), who in the sixteenth century created for medicine a solid foundation by transforming anatomy into an exact science, found that the action of the heart might be continued for some time by inflating the lungs of animals with air after the chest cavity was opened, and published, in 1543, his discovery of artificial respiration. Secondly, R. Columbus (4), in 1559, by direct experimentation on living animals discovered the pulmonary circulation. In fact, from Galen's time till Harvey's great discovery, with these exceptions little experimental work was done and during most of this period medicine instead of advancing towards a science became more and more the plaything of theorists and impostors.

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Harvey (5) following the practice of Vesalius and Columbus, of making anatomical examinations of the living parts according to experimental methods, established, in 1620, his great doctrine of the circulation of the blood. As it has been frequently denied that his discovery rested entirely on animal experimentation, I have given in the Appendix his own quaint and detailed account of it, which I am sure is quite enough to silence all question on this point. By a long series of carefully conducted experiments on animals and by that alone, he unravelled the system of the circulation and established a discovery which, more than any other, influenced the future of medicine and surgery. It needed only the microscopic demonstration of the capillary circulation by Malpighi, (6) and his demonstration of the circulation in the lung of a living frog, to make the solution of the great vital problem complete.

The next series of important and epoch-making experiments on animals were applied by Galvani (7) and Volta (8) to the nervous system. Each investigator was in error in his explanation of the results obtained by his researches, but their great discovery was not lost on account of the wrong interpretation which they gave to it, for their experiments on the electric condition of the nerves and muscles of animals established an epoch in the history of the physiology and pathology of the nervous system, which led to brilliant results a century afterwards.

The first attempt to continue life for an indefinite period by artificial respiration was made by Robert Hook (9), in 1664. He showed that "by inflating the lungs of animals with a bellows and then allowing them to collapse, an artificial respiration might be established which could be kept up for a long period." Artificial respiration according to Hook's method was afterwards practised by Brodie (10) and others for the purpose of studying the action of the heart and blood vessels and for resuscitating asphyxiated animals. The principle established by these experiments on animals was later applied to the human subject and is now a recognized means of preserving life in cases of asphyxia and of resuscitating the newly born.

The countless experiments on living animals, carried on during the seventeenth century in all the medical centres of Europe, on the action of the heart, the circulation, absorption, secretion and respiration produced a fund of knowledge without which the brilliant advances of the eighteenth century would have been impossible.

By experiments on birds, frogs and insects, Boyle (11) near the close of the seventeenth century (1670) showed that atmospheric air is necessary to the maintenance of life, as he found that air which had

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been breathed by animals for some time, became finally unfit for respiration so that the animals died.

Priestley (12) continued Boyle's experiments on air vitiated by respiration, establishing the fact that by growing plants in the vitiated air, it becomes regenerated and is again fit to breathe. It remained, however, for Lavoisier (13), following Hook's and Priestley's experimental methods, to establish at that time the true composition of atmospheric air and to develop the real basis of respiration; viz.: the absorption of oxygen and the exhalation of carbon dioxide. This discovery opened an entirely new field in the study of respiration and laid the foundation of our present knowledge of the respiratory processes.

The injection of fluids into the blood vessels of animals was first performed by Dr. Christopher Wren (14) of Oxford. He employed an infusion of opium and produced narcotism in the injected animals. His experiments in this line were soon followed by those of Richard Lower (15), who, in 1666, performed the first transfusion of blood in animals and the following year Dr. Denis (16) performed the same experiment on man. Mr. Boyle (17) afterwards elaborated the method of transfusing blood from one animal to another, and showed that death from hemorrhage might be prevented by such transfusion. These experiments led later to the establishment of the practice of transfusion for certain conditions of bloodlessness, a principle which to-day occupies a prominent place in life-saving methods.

After Galen's experiments on the nervous system of animals, the labors of investigators were chiefly confined to anatomical studies of the nervous system and little or no advance was made in the knowledge of its function until the middle of the eighteenth century, when Haller proved by numerous experiments in cutting and irritating nerves, "that all motion in the human body proceeds in a great measure from the brain and its annexed cerebellum and spinal marrow." He also demonstrated that when the peripheral end of a severed nerve is irritated the muscle to which it is distributed contracts. Soon after, Sir Charles Bell (18) commenced his experiments on the spinal cord and nerves of animals in order to determine the functions of the cerebrum and cerebellum, but after long labor reached no satisfactory result. Ten years later, however, while experimenting on facial nerves, in his attempts to demonstrate the existence of a great system of respiratory nerves separate from those of sensibility and voluntary motion, he established the important fact that the seventh cranial nerve is a nerve of motion and the fifth a nerve of sensation. This discovery has been fruitful of practical results both in medicine and surgery.

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Although Bell did not believe in animal experimentation as a source of knowledge, and employed it only to prove or confirm his anatomical studies, nevertheless his experimental work is the only part of his labor which has remained. The classifications and fascinating theories which he so ingeniously constructed on the basis of his anatomical studies are hardly known to neurologists of the nineteenth century.

Magendie (19) inaugurated the present century by a series of most brilliant experiments. He recognized the dangers of adopting theories based on imperfect knowledge and devoted himself accordingly to eliminating these imperfections by experiments on living animals. "The love of knowledge for its own sake was the impulse which dominated all his work. He claimed that all science was inductive and consequently founded on experiment and maintained that the science of life necessitated animal experimentation."

By a series of carefully conducted experiments on the spinal cord, in which he divided successively the anterior and posterior roots of the spinal nerve, he demonstrated the difference between the motor and sensitive nerve roots. In this way the distinct endowment of the two kinds of nerve fibres was established. Once placed on this footing the study of nerve physiology was greatly increased in efficiency and extent. Thus, by applying the Galvanic stimulus to a spinal nerve above and below the point of section, its mode of action was determined by the excitability of its motor and sensitive fibres. He employed the same methods in the study of the cranial nerves, both externally and at their roots. In fact every branch of inosculation was scrutinized by the same means.

It is hardly possible to estimate the importance of the change thus introduced into the study of the functions of the nervous system and the facilities which were thus supplied for further investigation. "This distinction between the spinal nerve roots was of the utmost importance, for it indicated a general plan of arrangement for the nervous system throughout the body. It became immediately a subject of criticism and verification for all the leading investigators of Europe and the result was a complete acceptance of Magendie's discovery." Magendie not only cleared up much that was vague and uncertain in the physiology of the nervous system, but he established methods of experimenting on the action of medicinal agents and was the first to demonstrate conclusively that "poisons act on the spinal cord through the circulation and not by means of the lymphatics and nerves." His results, obtained by experiments on animals with strychnine, quinine, iodine and a long list of medicinal substances, enabled him to lay the foundation of the doctrine that remedies exert their special action

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upon special structures and organs, a doctrine which was afterwards further developed by Claude Bernard and is now the accepted view regarding the action of all medicinal agents. He further demonstrated so thoroughly and clearly the action of strychnine on the spinal cord, that subsequent investigations have added but little to his results.

Magendie entered upon the investigation of all subjects with a sort of skepticism that demanded proof and counter-proof. In his studies of the functions of organs he was forced to experiment on animals and we may rightly regard him as the originator of the modern system of animal experimentation. Following the path which his great teacher had made so brilliant, Claude Bernard (20), by dividing the sympathetic nerve in the neck of the rabbit, observed that the bloodvessels in the ear of the corresponding side became enlarged and demonstrated by a series of similar experiments that the size of the bloodvessels is under the control of particular nerves which cause them to contract and dilate.

I demand from all opponents of animal experimentation recognition of the following fact. In all this long list of investigations not a single experiment was directed immediately to the discovery of a cure for a disease, but solely to the determination of physiological functions and the normal action of the vital processes, as indicated under the first head of our classification, and to defining the specific influence of given substances upon healthy living organisms, as indicated under the fourth head of our classification. It would have been impossible at that time even to guess just what valuable results were to come from these discoveries. Yet these experimentators fully understood, what every one must understand who expects to comprehend the purpose of medical science, that the practise of medicine is by principle and not by precept. Each worker recognized that the truth he sought was only a part of medical science and each by his discoveries marked an epoch in that science.

It were a task for days even to tell the things we are now able to do upon the basis of Bernard's discovery of the Vaso-motor nerves, and the story of the specific action of drugs is no shorter. It is probably a conservative statement to say that, excluding the medicinal foods, ninety per cent. of all our medication is made definite and valuable by this principle alone, which occupies the same position toward medicine as does Newton's Law of Universal Attraction toward Physics.

Although Brodie (21) had done much experimental work on the action of medicines without reaching any satisfactory results, it was not until Claude Bernard applied *his* experimental methods that the true action of drugs was fully understood. His work on

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Digitalis offers a most excellent illustration of the relative values of the experimental and clinical methods of study. Numerous observers had previously recognized that digitalis made the heart's action slower and therefore regarded it as a cardiac sedative. This conclusion Bernard proved to be the direct opposite of the truth, showing, by experiments on animals in which the drug was the only disturbing force, that its effect is not sedative, but on the contrary, stimulating and tonic, rendering the action of the heart more powerful and increasing the tension of the bloodvessels. The rules for its use in disease were thereby revolutionized and the results obtained by the use of this drug in so many diseased conditions were for the first time made certain. The necessity of investigating the action of drugs upon animals, in which the experiments could be controlled and varied, was thus conclusively proven by Bernard's work; and his methods were soon adopted by other investigators, by whom our knowledge of the action of remedies has been made definite to a degree that could never have been attained by mere observations of their effects upon man.

Thus, modern Therapeutics, emancipated from the bondage of empyricism, stripped of its chains, in which every link of personal interpretation differed from its fellows, no longer wounding friend as well as foe by aimless blows in the dark, stands forth a young but growing Hercules, bearing in place of the old barbaric club a magazine rifle with telescopic and microscopic sights, the ammunition box of which holds not drugs alone but a manual of directions wherein are written the truths which our modern priests, offering sacrifices on the altars of science, have given to mankind to save them from their infirmities.

Still the tale goes on; Magendie, Bernard and Longet established by their experiments the doctrine of recurrent sensibility, which was followed by the great discovery of Marshall Hall (22) of the reflex action of the spinal cord. He observed that after the removal of the brain in animals the limbs were still capable of motion and he showed by further experiment that the spinal cord acts independently of the brain as a medium of communication between the integument and the muscles. The same form activity was afterwards found to be very widely extended in the nervous system. Legallois (23) and Flourens (24) showed that the medulla has its own centres of reflex action and is either directly or indirectly an essential to to the continuance of life.

There is to-day no more important department of nerve physiology than that in connection with the vaso-motor nervous system. It has been studied and developed by many observers but, as just mentioned, was practically established by Bernard's experiments. It solved not

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only some of the most important and difficult problems of physiology, but made intelligible many unexplained pathological changes. The relation of disturbances of the surface circulation to diseases of the internal organs, the mechanism of local congestion. The recovery of nerves from the exhaustion of over stimulation, by rest. The varying effects produced by different kinds of electric stimuli and much similar knowledge mark the fruit of a long series of experiments made along the line so clearly marked out by Bernard. Each one of these series rested on the work of some preceding experimenter; together they form a continuous line of development which can be traced directly to the work done in Galvani's laboratory in 1789.

We turn now to experiments falling more directly under our third class. It is extremely suggestive as well as interesting to note that the order in which these discoveries came most clearly indicates that all investigators were working toward the relief of human suffering as their one great object. Anatomy and physiology could not be ignored, but pathology and etiology were forced to give place to therapeutics.

John Hunter (25) in 1785, by his experiments on the arteries of dogs, established the fact that injuries to healthy arteries were soon repaired and that ulceration of arteries after ligature only occurred in such as were primarily diseased at the point of ligation. These experiments led him to apply the ligature, for the cure of aneurism, to the healthy portion of the artery above the point of dilation. For more than a century his experiments on canine legs have borne fruit an hundred-fold in saving human lives and limbs.

Hunter first learned by experiments on pigeons and young pigs, "that the growth of bone takes place mainly from the exterior and is probably produced by the nutritive power of the periosteum." Subsequently, this question was further examined experimentally by Howship (26), Flourens (27), Heine (28), Murray and others. Mr. Syme (29) endeavoured to ascertain "whether the periosteum possesses the power of forming new osseous substance independently of any assistance from the bone itself. He extirpated the middle portion of the radius of a dog with its periosteum and found, as Sir Astley Cooper had previously done, that after the recovery of the animal there was no bony union, but a ligamental band running from one extremity to the other."

Leopold Ollier (30), by transplanting portions of the periosteum, demonstrated the power of the membrane to produce new bone and showed that in the resection of bone, if the periosteum is left, new bone will be developed in from one to three months. Following the

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teachings of his animal experiments, he introduced the operation of resection for diseased bone, one of the most important discoveries in surgery which inaugurates a new era in surgical procedure. The practical results of his discovery are to-day fully approved by surgeons in the management of injuries or disease of the bones and joints.

As we witness some capital operation performed at the present day without pain, almost bloodless, followed neither by fever nor suppuration, we may ask how far these great results are due to experiments on animals. The anæsthesia of chloroform was discovered through experimentation on a low form of animal life, the ant. The illustrious Simpson practised and perfected his use of chloroform on animals before he anæsthetised his first patient. While its discovery as an anæsthetic cannot be claimed as altogether due to experiments on animals, its great uses in surgery would be incomplete but for such experiments. The same is true of that other great alleviator of pain, the hypodermic, which was first used by Mr. Rand (31) on his sporting dogs. He says "I feared that hypodermic injections might excite suppurative inflammation in the subcutaneous tissue, but when I found from experiments on dogs that this was not the case, I gained confidence that justified their use on man."

Whether Ambrose Paré made experiments on animals before using the ligature on man is not clear, but all the steps which were necessary to perfect this discovery and all the other means for arresting hemorrhage from bleeding vessels, have been the result of experiments on animals by Hunter (32), Jones (33), Benj. Travers (34), Bryant (35) and a long list of eminent surgeons during the past century.

Robert McDonnell states that great as are these triumphal epochs in surgery, which have rendered operations painless and bloodless, the practical surgeon of to-day values even more highly those antiseptics which render convalescence after operation free from fever and suppuration.

These three great epochs alone are sufficient to exalt animal experimentation to the first place as a means of scientific advancement, nor can they be obscured by the the multitude of brilliant discoveries that are showering upon us with a bewildering rapidity in these latter days.

I cannot help again calling your attention to the peculiar significance of the fact that etiology, which by right should have come first in the study of disease, has hitherto marched but haltingly in the rear, because it must depend upon animal experimentation alone for its development. It is unfortunate that a false sentiment has sought by every possible means to retard and check the progress of those, who essayed this path of investigation.

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The first step in this important field of research was taken in 1850, when a commission of the medical association of the "Eure et Loir" proved that splenic fever could be communicated from one animal to another by innoculation; and the first hint of bacteriological study was given when Davaine (36) and Rayer found constantly in the blood of animals so innoculated little thread-like bodies.

At about the same time Prof. Virchow (37) furnished definite knowledge of the inception and prevention of parasitic diseases by a series of experiments on animals to determine the origin, nature, mode of development and communicability of trichinosis. Through these investigations the medical profession first became aware of the existence of a fatal and heretofore unknown disease, and were at the same time made acquainted with its source and manner of production.

M. Villemin (38) inaugurated a new and important era in medicine when he established the fact that tuberculosis is an infectious disease. By laboratory experiments he found that general and fatal tubercular infection is produced in animals when they are inoculated with crude tubercular matter from the human subject. His experiments in this line were soon confirmed by other investigators and led directly to all the far-reaching results, determining the modes by which tubercular infection can be propagated. Villemin's discovery revealed endless possibilities and was followed by similar investigations of many infectious diseases.

The invaluable studies of Pasteur (39) introduced us into "a new world of strange knowledge." His insight into the doctrine of fermentation carried him far beyond the agencies of microscopic organisms in fermentative processes. He not only isolated and obtained pure cultures of these organisms but also studied their life-history, and by methods which had served him in all his previous investigations, placed bacteriological science on a firm basis.

He demonstrated that, if the cause of an infectious disease be a self-multiplying germ from the outside world, the habits of the living enemy can be studied in its outside relations and that definite knowledge may thus be obtained as to its biological affinities. No work has ever promised greater things to the world than does that of Pasteur.

The knowledge obtained by observation, previous to the middle of the present century, concerning the changes effected by disease now began to be examined from a new point of view. Pasteur's methods of experimentation were adopted and practised by investigators all over the world and his doctrine, that the cause of infectious diseases was to be sought in self-multiplying germs, was everywhere accepted.

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The crowning glory of Pasteur's work came with the discovery of attenuation of bacterial toxic products, the possible results of which defy the imagination. In the record of human industry there is no work of richer or grander promise. Yet it is a work which, from the very nature of the case, was possible only by experiments on living animals.

The endless possibilities of research which the new doctrine of infection suggests to the mind of the pathologist was the beginning of the most brilliant era in the science of medicine since it has existed. It would not be possible in the time allotted to me even to refer to all the investigations which have been made along the line so clearly defined by Pasteur.

I must pass by even the list of diseases over which victory has been made possible, in order that I may speak briefly of those in which practical results are already attained or in sight. The time is here when abstract scientific principles are bearing concrete fruit in the prevention and cure of disease.

The application of Pasteur's doctrine by Mr. Lister to the antiseptic treatment of wounds, an application which was enforced by Mr. Lister's eminent skill as an experimenter, has been a full confirmation of this principle. The germs which Pasteur imprisoned in his test-tubes were liberated by Lister's genius. Together they have put to flight the horrors of surgery and forever stand guard when the surgeon plunges his knife into the sleeping flesh or binds up the torn and mangled body.

The discovery of the bacillus tuberculosis by Koch (40) marks another brilliant epoch in medical science. The details of the work by which he reached this great discovery are too familiar to be repeated here, but it was only accomplished by Pasteur's method of continuous animal experimentation. It has not merely revolutionized our knowledge of tuberculosis, but has enabled us to understand and explain the morbid processes of tubercular disease. The stimulus which Koch's work gave to investigation in all countries is bearing fruit in the department of preventive medicine and therapeutics. The light which flashed from his laboratory was the dawn of hope that is already breaking into the day of certainty, when we shall hold a prevention and cure of tuberculosis in our grasp.

Following the line of Pasteur's work on the attenuation of bacterial toxic products, Kitasato (41) and Behring (42) in their experiments on the immunity and cure of tetanus have given definite knowledge of the prevention and cure of infectious diseases, which goes far towards the realization of the hope inspired by the work of Koch on the toxic products of the bacillus tuberculosis.

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Within the past two decades animal experimentation has accomplished more in the field of cerebral localization than all the preceding centuries of carefully recorded cerebral symptoms studied in the light of post-mortem observations. It has opened to surgeons an entirely new field of operations. Until the middle of the present century the brain was described as a single organ and physiologists attributed to its functions no special localization. To-day the areas of motion and of special sense, and to a limited extent the mental areas also, have been definitely placed. Most, if not all, of this knowledge has been derived from experiments on the brains of living animals. From the time when Fritsch and Hitzig (43) reported their faithfully recorded and startling experiments in this new field of research, giving such complete details of their experimental procedure that other investigators could easily follow and test their accuracy, numerous workers have been adding to our knowledge, until we now have definite and safe rules by which we may localize many cerebral lesions. Any discrepancies which have been claimed as invalidating this line of work Dr. Ferrier (44) states will be found on careful examination to be more apparent than real and that experiments on animals under conditions selected and varied at the will of the experimenter are alone capable of furnishing precise data for sound inductions as to the functions of the brain in its various parts.

The surgeons have not been slow to make this knowledge practical in skillfully-devised operations. The region beyond the skull is no longer forbidden ground. The hopeless because powerless inactivity, that once watched in wondering silence the meaningless signs of conflict within the mind's temple, has now given place to a keen activity that boldly enters the inner court to seize the offender even at the base of the altar.

As we review the history of the experimental work which has placed medicine in the list of the sciences we are impressed with the fact that while each line of research was carried on for its own results, nevertheless most of the discoveries remained apparently barren until subsequent discoveries invested them with an unexpected importance.

The fatal error of the critics of experimental work is their demand that every experiment shall bear fruit immediately; few if any of them seem to be familiar with scientific methods. They constantly wander from the real issues to the moral or morbidly sentimental aspect of the question.

It seems evident from the history that I have spread before you in this brief and fragmentary manner, that most, if not all, of the real advances in medicine have been made possible through experimenta-

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tion. The only point open to discussion then, from either moral or ethical point of view, is what price should be paid by the world for the benefit received. This review of what our profession has thus far done to serve mankind in this field is not a plea for mercy, not a bribe to tempt a captious public. My voice has proved a recreant servant if any tones of doubt or fear have marred this exposition. Its every part is cause for pride. In such a sketch, however cursory it may be, each point reflects the light of noble purpose and overflows with promise of better things to come. So long as the moral and spiritual development of mankind remains the supreme purpose of creation, medical science can claim equal honors with the science of God and in the conflict with physical evil must be the first to meet the foe. Until Infinity repeals the edict which gave man power over all created things, the right to claim the service of the brute creation, although it takes the life of the animal or tears it limb from limb, can never be denied to him who devotes his life to the service of mankind.

In this defence of animal experimentation results have not been made prominent with any purpose to conceal methods. We are fully prepared to count the cost and to meet the question "does the end justify the means?" As devotees to medical science, we yield precedence to none in honesty and lawfulness of purpose, or faithfulness of service in the bitter conflict humanity ever has waged and ever must wage against pain and disease. We too have hearts that love and pity, that ache and sometimes even break beneath the loads they bear. We glory in our experimental work because we know the tenderness of cruelty, the balm of pain; the life whose birth is only in the throes of death. Must then our conflict cease? our weapons be laid aside because selfish ambition has now and then made fiends of men? Since philosophers first learned to trace "with their golden pen on the deathless page" heroic deeds of men, humanity has never failed to offer its first homage to those who gave their lives for others. As the servants of such a science we can fearlessly appeal to all intelligent men for a just criticism. From the ignorant we expect to receive only censure, but from those who in "the valley of the shadow of death" have learned to know what manner of men we are, I have faith to believe the reply will come; *We have trusted you with the lives of our loved ones; we entrust to you God's dumb creation.*

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APPENDIX.

(1)

Aristotle,* History of Animals. Aubert and Wimmer's Edition.

Book II, Chapter II, Section 44, vol. i, p. 272. (In the Chameleon) "when cut open, respiration continues active for a long time, and there occur very slight movements in the heart; and contractions take place not only, and especially, in the region of the ribs, but also in the other parts of the body."

Book III, Chapter XIX, Section 90. "In no animal has the blood any feeling when touched, any more than the excretions in the belly; nor has the brain or marrow,† when touched, any feeling."

(2)

On Practical Anatomy and Experimental Physiology. De Anatomicis Administrationibus. Galeni Opera Omnia, Kuhn's Edition, vol. ii, pp. 651–706.

An in Arteriis Natura Sanguis Contineatur. Galeni Opera Omnia, Kuhn's Edition, vol. iv, p. 708. Is blood naturally contained in the Arteries?

"For we have often exposed the large arteries convenient for this purpose, and asked the disciples of Erasistratus whether the artery thus exposed did not seem to contain blood. They were obliged to confess that it did, both because Erasistratus asserted that the blood passed into the arteries when they were uncovered, and because the fact was evident to the senses; for having placed ligatures on both ends of the inclosed portion of the artery, and made an incision into the vessel, between them, I showed that the artery itself was full of blood."

Book VIII, Chapter II. "There is a form of respiration which is considered to be a natural (i. e. involuntary) as opposed to a psychical (i. e. voluntary) function, and in which the lower parts of the chest and hypochondria are seen to be plainly in motion, but the upper parts sometimes not at all, and sometimes obscurely. This is accomplished by the diaphragm alone, which is a muscle not only in structure but in function also."

"But our teachers were wrong in believing the diaphragm to be the sole mover of the chest in respiration, expanding it when itself in contraction, and when itself in relaxation permitting the chest to collapse; for they did not explain how we are able to blow out vigorously or to vocalize. They thought that even the ample movements of the chest, which we see in running and in all such sharp gymnastics, are accomplished by the energy of the diaphragm." Galen goes on to say that no account was taken of the intercostals or muscles of forced breathing.

* The extracts from Aristotle, Galen, Vesalius, Columbus, and Malpighi were kindly furnished by Prof. J. G. Curtis from his library.

† The word marrow μυελός is used by Aristotle indifferently for the marrow of the bones and the spinal cord, the functions of which latter were unknown to him.

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Book VIII, Chapter III. The technique of the dissection and experimental physiology of the muscles of respiration is here given, and the anatomy of the two sets of intercostals is described. The experiments are done on pigs because their voices are so strong that effects can be well observed.

If the external intercostal muscles are divided first and then the internal, the voice and forced breathing are abolished.

Galen uses large pigs, so that opening the pleura may be avoided, and advises one to practice well on the cadaver before experimenting. This demonstration of the use of the intercostal muscles Galen claims as new and original with him.

Book VIII, Chapter IV. Next he proves that the intercostal muscles owe their power to the intercostal nerves. For this purpose all these nerves are exposed near the spine and ligatured, but not so tightly as to bruise or sever the nerves. The ligatures on the nerves, if moderately tight, (1) paralyze the intercostal muscles, (2) prevent forced breathing, (3) abolish the voice. All these functions return when the ligatures are removed.

Book VIII, Chapter V. If the nerves now called the vagi are destroyed a hoarse sound like a snore may still be produced; but if the intercostal muscles are paralyzed, there is no hoarse sound at all.

Paralysis of the intercostal muscles may be effected not only by (1) section of their fibres, and (2) injury of their nerves, but by (3) excision of their ribs, and by (4) section of the spinal cord at the beginning of the back, between the seventh cervical and first dorsal vertebræ. This section of the spinal cord is found to paralyze every muscle below except the diaphragm, and also to render the animal voiceless. In an animal breathing only by the diaphragm, after section of the spinal cord at the beginning of the back, section of the phrenic nerves is seen to be followed by the contraction of certain accessory muscles at the upper part of the chest. In another animal Galen cut the phrenic nerves in the neck, and found that while the diaphragm was paralyzed, the intercostal muscles continued to act. In experiments where the spinal cord was cut at the beginning of the back, the upper and lower parts of the thorax might be seen to move, the lower by means of the diaphragm, the upper by means of accessory muscles.

In Book VIII, Chapter VI, the technique of cutting the spinal cord is given minutely. Galen sometimes used sucking pigs for this purpose. "When the spinal cord is divided longitudinally from above downward, directly in the middle line, not one of the intercostal nerves is paralyzed, either to the right or left, nor the lumbar nor crural nerves. But if the cord is divided transversely, only one-half way on either the right or left side, all the nerves of the injured side are immediately paralyzed." Hemisection of the cord reduces the volume of the voice one-half; complete section produces complete aphonia.

Book VIII, Chapter VII. Excision of the ribs destroys forced breathing and the voice as much as section of muscles or of nerves. Galen describes the technique of this excision. It must be subperiosteal and he cautions against piercing the pleura. The impression received is that *not all* of the ribs are to be removed.

Book VIII, Chapter IX. Section of the spinal cord at its commencement, or below the first, second or third cervical vertebræ totally paralyzes respira-

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tion, and the whole of the body below. If below the sixth vertebræ the animal can still use the diaphragm, and if cut further down, other muscles of respiration remain intact: the lower the section, the greater the number.

(3)

Andreae Vesalii, Bruxellensis, 1543. De Humani Corporis Fabrica. Lib. Septem, DeVivorum Sectione.

"In order that the life of an animal may be restored, an opening should be made in the trunk of the aspera arteria (trachea), into which a canula of reed is inserted and this blown through. For on slight inflation in the living animal the lung swells up to the size of the thoracic cavity, and the animal breathes after a fashion. The heart then resumes its force, and its motion varies with beautiful diversity. The lung being inflated from time to time the motion of the heart can be well examined, both by sight and touch, and the trunk of the great artery [aorta] which extends along the back can be examined also in the thoracic cavity, or as far as the lumbar vertebræ.

"Nothing appears more manifest to you than the rhythmic beat of the heart and arteries, which being observed for a time, the lung should be inflated again. By this artifice, than which nothing I have discovered in experimental physiology [literally anatomy, but with more than its present meaning], pleases me more, much knowledge of the changes in the pulse may be obtained. For when the lung has remained flaccid for a time, the pulse or motion of the heart and arteries is seen to be undulating, creeping or vermicular, but the lung being inflated the pulse becomes large and rapid and shows remarkable inequality.

"I may say that this is the experiment by which I demonstrate to the best advantage to the candidates in medicine the nature of every kind of pulse."

(4)

Realdi Columbi, De Re Anatomica. Venetiis, MDLIX. p. 177, l.10–20.

"There are two cavities in the heart, that is two ventricles, not three as it seemed to Aristotle. One of these is to the right, the other to the left; the right is much larger than the left and contains natural blood, while the left contains the vital. Moreover, it is easy to satisfy one's self by observation that the substance of the heart which encloses the right ventricle is thin, and that the left is thick; this is partly for the sake of equilibrium, and partly in order that the vital blood which is very thin may not exude. For between these ventricles there is a septum through which almost all think that a passage is open for the blood from the right ventricle to the left, and that the easier, because on the way the blood is attenuated for the purpose of the generation of the vital spirits. But they are much out of the way; for the blood is carried through the pulmonary artery to the lung, and there undergoes its attenuation; thence along with air it is carried through the pulmonary vein to the left ventricle of the heart; which fact no one has hitherto noticed, or left recorded, although it is most worthy of the attention of all."

p. 178, l. 31-38. "Indeed I think just the opposite; namely, that the pulmonary vein is to carry blood mixed in the lungs with air to the left ventricle of the heart; which is as true as truth itself; for if you will examine not only in the cadaver, but likewise in the living animal, you will find always this [vein] filled with blood, which would by no means be the case if it were merely for air and vapors."

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p. 224, l.16–21. "Verily I beseech you, oh candid reader, studious of the learned, but most studious of the truth, to experiment upon animals and to dissect them alive; try, I say, whether what I have said agrees with the facts; for in these animals you will find the pulmonary vein full of blood, not filled with air or smoky fumes, as they call them, please God! Only the pulse is lacking."

(5)

Harvey's works. Sydenham Edition, p. 19.

"When I first gave my mind to vivisections as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think, with Fracastorious, that the motion of the heart was only to be comprehended by God. For I could neither rightly perceive at first when the systole and when the diastole took place, nor when or where dilation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning; so that the systole presented itself to me now from this point, now from that; the diastole the same; and then everything was reversed, the motions occurring, as it seemed variously and confusedly together. My mind was therefore greatly unsettled, nor did I know what I should myself conclude, nor what believe from others; and I was not surprised that Andreas Laurentius should have said that the motion of the heart was as perplexing as the flux and reflux of Euripus had appeared to Aristotle. At length, and by using greater and daily diligence, and collating numerous observations, I thought that I had attained the truth, that I should extricate myself and escape from this labyrinth, and that I had discovered, what I so much desired, both the motion and use of the heart and the arteries. Since which time I have not hesitated to expose my views upon this subject, not only in private to my friends, but also in public, in my anatomical lectures, after the manner of the Academy of old."

From a Biographical Sketch of Harvey in the Philosophical Transactions, London, Abridgment, 1809, p. 319.

"He shows, by experiments made on living animals, that the motion of the heart is performed by the contraction of its muscular fibres; that the auricles contract first, and thereby propel the blood into the ventricles; then the ventricles contract, whereby the blood is driven into the arteries; being prevented from returning into the auricles by the situation and connection of the valves. Now as by repeated contraction of the ventricles more blood is constantly propelled into the arteries than can be supplied by nourishment thrown into the veins (as appears upon calculation), and as moreover the arteries cannot receive blood through any other channel but the veins; it follows either that the veins must be quickly emptied, and the arteries on the contrary every moment more and more distended, which however is not the case; or that the blood must flow back again from the arteries into the veins, by certain secret passages, or by pores of the flesh, or by mutual anastomoses of the arteries and veins. He demonstrated that the last mentioned communication takes place in the lungs. Again: as along the course of the arteries more blood is sent from the heart to all parts of the body than is necessary

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for the nourishment of those parts, he infers that the superfluous blood is returned by the veins (that they may not be left empty) from the fact, that no blood is found in the veins if the great artery be tied. On the other hand if a ligature be placed on the vena cava at the place where it joins the right auricle, it will immediately become distended in a very surprising manner. Moreover it must be evident to every one (he observes) who considers the situation and connection of the valves, that the blood passes from the smaller branches of the veins into their trunks, and from thence to the heart."

(6)

M. Malpighi. Opera Omnia. Lugduni Batavarum. Apud Petrum Vander Aa. Bibliopolam MDCLXXXVII, Vol. II. De Pulmnibus Epistola II, p. 328.

"These things being apparent as regards the mere structure and connection [of the lungs], microscopic observation discovers still more wonderful things. For if the heart is still pulsating, the contrary motion of the blood is to be observed in the vessels, although with difficulty, so that the circulation of the blood is plainly to be detected, and can be made out even more successfully in the mesentery and in the other large veins contained in the abdomen. The blood then [entering] an [air] cell by the impulse through the arteries, as one or another conspicuous branch passes by or ends in a cell, rains down, finely broken up, as though poured out, and, thus multitudinously divided, loses its ruddy color, and, carried sinuously about, is scattered on all sides until it lands at the walls and angles [of the air-cells] and the branches of the veins which take it up again.

"Nothing more could be seen in the living animal operated upon. Hence I had believed that the body of the blood broke out into an empty space, and was gathered together again by and open-mouthed vessel and by the help of the structure of the walls [of the air-cells]. The basis for this view was offered by the tortuous movement of the blood, diffused as it was in various directions, and by the gathering of it together at a definite point; nevertheless, my faith was shaken by the [appearance of the] dried lung of a frog, which, as it happened, had retained the redness of the blood in its smallest parts (vessels as I found them afterward); for by the aid of a more perfect glass there appeared to the eye no longer points which looked like the skin called *Shagreen*, but in place of them, minute vessels mingled together ring-fashion, and so great is the divarication of these vessels, as they spring here from vein and their from artery, that there is no longer any order preserved, but they appear as a net-work made up of the prolongations of the two [main] vessels. This net-work not only occupies the entire area [of the air cell]; but extends to the walls and blends with the efferent vessel, as I was able to observe repeatedly, although with great difficulty, in the oblong lung of the tortoise, which is likewise membranous and diaphanous. Hence it was made apparent to the senses that the blood was divided up and flowed through tortuous vessels, and was not poured out into spaces, but moved always through little tubes and was scattered owing to the multitudinous bends of the vessel. Nor is it any new thing in nature for the terminal mouths of vessels to be joined together, since in the intestines or other parts the same plan is followed, and, even more wonderful though it may seem, the upper ends of veins are joined with the lower ends [of others] by anastomosis, as was very well observed by the most learned Fallopius.

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"In order, however, to obtain and verify the foregoing results, tie the turged lung with a string, at its junction with the heart, as it protrudes from an opened frog, and while it is every where abundantly flushed with blood; for such a lung when dried will continue to have its vessels swollen with blood, which then you will see exceedingly well by examining them against a horizontal sun with a microscope of a single lens. Or you may use another method in looking at the vessels. Place the lung upon a plate of crystal illuminated by the light of a lantern from beneath through a tube: employ for this a microscope of two lenses, and there will be visible to you vessels arranged in rings, and by means of the same disposition of instruments and light you will observe the movement of the blood through the said vessels; and, by varying the amount of light, you will be able to contrive for yourself other things which defy description by the pen.

(7 and 8)

Account of some Discoveries made by Mr. Galvani, of Bologna; with Experiments and Observations on them. In two letters from Mr. Alexander Volta, F. R. S. Professor of Natural Philosophy in the University of Pavia, to Mr. Tiberius Cavello, F. R. S. Read Jan. 31st, 1793. Philosophical Transactions, London, 1793, pp. 10–44.

The fact that these letters are written in Old and very bad French renders a certain freedom of translation necessary.

In speaking of the Commentary of Galvani, entitled "ALOYSII BONONIÆ GALVANI *de Viribus Electricitatis in Motu Musclari Commentarius*, 1791, 4to; de 58 pages, avec quatere grandes planches," Volta says "it contains one of the most beautiful and surprising discoveries, and the germ of many others.

(1) "Dr. Galvani, having dissected and prepared a frog in such a manner that the legs were attached to the spinal cord only by the exposed crural nerves, and having cut off the rest of the body, saw that he excited lively movements in the legs, with spasmodic contraction of all the muscles, each time that a spark was drawn from the conductor, not only on the body of the animal, but upon every other body, and in every direction (the legs being at a considerable distance from the large conductor of the electrical machine, and under certain other circumstances which I shall explain further on).

"The required circumstances were, therefore, that the animal thus dissected should be in contact or very near some sort of metal or other good conductor, sufficiently extended, and better yet, between two similar conductors, one of which should be turned to the extremity of said legs, or some one of their muscles, the other toward the spine or the nerves: it was also very advantageous that one of these conductors (which the author distinguished by the names of *nerve-conductor* and *muscle-conductor*) and preferably the latter, be in free communication with the floor. It is in this position, especially, that the legs of the frog, prepared as has been described, received violent shocks, and twitched and struggled with vivacity at each spark of the conductor from the machine, although it was quite far distant, and although the discharge was made neither on the nerve-conductor nor on the muscle-conductor, but on any other equally distant from them, having all communication for the transmission of such a discharge, for example, on a person placed in the opposite corner of the room."

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(5) "I applied myself with considerable attention to determine what was the least electric force necessary to produce these results in the frog intact and full of life, as well as one dissected and prepared in the manner described; which Mr. Galvani had omitted to do. I chose the frog in preference to any other animal because it is endowed with great vitality and is easily prepared. Moreover, I have made experiments upon other small animals, with the same end in view, and with about an equal success. To properly estimate the value of the electric force, I thought it proper to subject the animal, destined for experiments of this kind, not to the return currents occasioned by the atmosphere, but to the direct electric discharges, now by a simple conductor, now by a Leyden jar, in such a manner that all the current should go through the body of the animal. To this effect I was careful to hold it isolated in some manner or other, and more often by fastening it with pins to two plates of soft wood, supported by glass columns."

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(8) "Thus we have, in the legs of the frog attached to the spinal column solely by the uncovered nerves, a new kind of electrometer; since electric discharges which give no indications with the ordinary machines, give marked signs with such an *animal electrometer*."

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(11) "Mr. Galvani did not stop here in these truly astonishing experiments on the frog; he extended them with success not only to other cold blooded animals but to birds; in which he obtained the same results by means of the same preparations; which consisted in disengaging one of the principal nerves from its envelope, where it entered a member susceptible of movement, arming such a nerve with a piece of metal, and establishing a communication, by means of a conducting arc, of the nerve and its muscles" [with the machine].

(12) "He also very happily discovered, and demonstrated in a very evident manner, the existence of an *animal electricity* in all or nearly all animals."

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(19) "Experiment A. I caught with forceps the ischiatic nerve a little below its insertion in the thigh, and applied wires, a piece of money or other metallic plate, a little higher up upon the same nerve, carefully dissected from its attachments, and held up by a thread, or supported by a plate of glass, a stick of "bees' wax, or of dry wood, or any other poor conductor. Then applying the body of a Leyden jar, very feebly charged, to said forceps, I carried the arc into contact with the other metallic plate, and saw that the discharge was made; which, though not strong enough to give the least spark, caused all the muscles of the thigh and leg to become convulsed and twitch more or less impetuously. This was true of the nerve throughout the entire leg, or any part of the nerve projecting beyond it, when in the course pursued by the current in its transit; though but a small portion of the nerve be irritated, this nevertheless, was sufficient to occasion contraction of the muscles."

(9)

An Account of an Experiment, made by Mr. Hook, of preserving Animals alive by blowing into their Lungs with Bellows. Philosophical Transactions, London, No. 28, p. 539.

"I did, therefore, heretofore give this Illustrious Society an account of an

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experiment I formerly tried of keeping a dog alive after his thorax was all displayed by the cutting away of the ribs and diaphragm, and after the pericardium of the heart was also taken off. But divers persons seeming to doubt of the certainty of the experiment (by reason that some trials of this matter, made by some other hands, failed of success), I caused at the last meeting the same experiment to be shown in the presence of this Noble Company, and that with the same success as it had been made by me at first, the dog being kept alive by the reciprocal blowing up of his lungs with bellows, and then suffered to subside, for the space of an hour or more after his thorax had been displayed and his aspera arteria (trachea) cut off just below the epiglottis, and bound upon the nose of the bellows."

(10)

The Croonian Lecture on some Physiological Researches, respecting the Influence of the Brain on the Action of the Heart, and on the Generation of animal Heat. By Mr. B. C. Brodie, F.R.S. Read Dec. 20th, 1810. Philosophical Transactions, London, 1811, vol. xi, p. 86.

"In making experiments on animals to ascertain how far the influence of the brain is necessary to the action of the heart, I found that when an animal was pithed by dividing the spinal marrow in the upper part of the neck, respiration was immediately destroyed, but the heart still continued to contract, circulating dark colored blood, and that in some instances from 10–15 minutes elapsed before its action had entirely ceased. I further found that when the head was removed, the divided blood-vessels being secured by a ligature, the circulation still continued, apparently unaffected by the entire absence of the brain. These experiments confirmed the observations of Mr. Cruikshank (Phil. Trans. 1795) and M. Bichat (Récherches Physiologiques sur la Vie et la Mort) that the brain is not directly necessary to the heart, and that when the functions of the brain are destroyed, the circulation ceases only in consequence of the suspension of the respiration. This led me to conclude, that, if respiration were produced artificially, the heart would continue to contract for a still longer period of time after the removal of the brain. The truth of this conclusion was ascertained by the following experiment.

"Experiment 8.—I divided the spinal marrow of a rabbit in the space between the occiput and the atlas, and having made an opening into the trachea, fitted into it a tube of elastic gum, to which was connected a small pair of bellows, so constructed that the lungs might be inflated, and then allowed to empty themselves. By repeating this process once in five seconds, the lungs being each time fully inflated with fresh atmospheric air, an artificial respiration was kept up. I then secured the blood vessels in the neck, and removed the head, by cutting through the soft parts above the ligature, and separating the occiput from the atlas. The heart continued to contract, apparently with as much strength and frequency as in the living animal. I examined the blood in the different sets of vessels, and found it dark colored in the venæ cavæ and pulmonary artery, and of the usual florid red color in the pulmonary veins and aorta. At the end of 25 minutes from the time of the spinal marrow being divided, the action of the heart became fainter, and the experiment was put an end to."

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(11)

New Pneumatic Experiments about Respiration, by the Hon. Robert Boyle. Phil. Trans. Lond. No. 62, p. 2011.

The account of these experiments is so long that it will be impossible to reproduce it here. The headings or "Titles" of the different chapters will give a sufficient insight into the experiments themselves. The air-pump, then newly invented, was employed in nearly all of them.

"THE FIRST TITLE.

Observations on the lasting of Ducks included in the Exhausted Receiver.

THE SECOND TITLE.

Of the Phænomena afforded by Vipers in an Exhausted Receiver.

THE THIRD TITLE.

Of the Phænomena afforded by Frogs in an Exhausted Receiver.

THE FOURTH TITLE.

Of the Phænomena afforded by a newly kittened Kitling in the Exhausted Receiver.

THE FIFTH TITLE.

Some Trials about Air usually harbored and concealed in the Pores of the Water, &c.

THE SIXTH TITLE.

Of the Phænomena afforded by Shell Fishes in an Exhausted Receiver.

THE SEVENTH TITLE.

Of the Phænomena of a Scale Fish in an Exhausted Receiver.

THE EIGHTH TITLE.

Of two Animals with large Wounds in the Abdomen, included in the Pneumatic Receiver.

THE NINTH TITLE.

Of the Motion of the separated Heart of a Cold Animal in the Exhausted Receiver.

THE TENTH TITLE.

A Comparison of the Times wherein Animals may be killed by Drowning, or withdrawing of the Air.

THE ELEVENTH TITLE.

Of Accidents that happened to Animals in Air brought to a considerable degree, but not near the utmost of Rarefaction.

A digressive Experiment concerning Respiration upon very high Mountains.

THE TWELFTH TITLE.

Of the Observations produced in an Animal in Changes as to Rarity and Density made in the self-same Air.

THE THIRTEENTH TITLE.

Of an unsuccessful Attempt to prevent the Necessity of Respiration by the Production or Growth of Animals in our Vacuum.

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THE FIFTEENTH TITLE.

Some Experiments, showing that Air, become unfit for Respiration, may retain its wonted Pressure.

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THE SEVENTEENTH TITLE.

Of the long Continuance of a Slow-worm and a Leech alive in the Vacuum made by our Engine.

THE EIGHTEENTH TITLE.

Of what happened to some Creeping Insects in our Vacuum.

THE NINETEENTH TITLE.

Of Phænomena suggested by Winged Insects in our Vacuum.

THE TWENTIETH TITLE.

Of the Necessity of Air to the Motion of such small Creatures as Ants, and even Mites themselves."

(12)

Joseph Priestley's Experiments on Respiration. Philosophical Transactions, London. Vol. lxii, p. 147. Read March 5th, 12th, 19th, 26th, 1772.

His first experiments were upon "fixed air" (carbon dioxide), and for the purpose he used "insects and animals which breathe very little" and frogs.

He then tried different methods for restoring air, in which candles had been burned, to its former state, such as the effects of heat, cold, and condensation.

p. 166. "Though this experiment failed, I flatter myself that I have accidentally hit upon a method of restoring air which has been injured by the burning of candles, and that I have discovered at least one of the restoratives which nature employs for this purpose. It is vegetation."

"On the 17th of August, 1771, I put a sprig of mint into a quantity of air, in which a wax candle had burned out, and found that on the 27th of the same month, another candle burned perfectly well in it."

"This restoration of air I found depended upon the vegetating state of the plant; for though I kept a great number of the fresh leaves of mint in a small quantity of air in which a candle had burned out, and changed them frequently, for a long space of time, I could perceive no melioration in the state of the air."

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p. 181. "That candles will burn only a certain time, is a fact not better known, than it is that animals can live only a certain time, in a given quantity of air; but the cause of the death of the animal is not better known than that of the extinction of flame in the same circumstances."

Priestley noticed that plants, put into air tainted by putrefaction, grew vigorously, and at page 193 he says, "This observation led me to conclude, that plants, instead of affecting the air in the same manner with animal respiration, reverse the effect of breathing, and tend to keep the atmosphere sweet and wholesome, when it is become noxious, in consequence of animals living and breathing, or dying and putrefying, in it."

He fully proved his conclusion by experiments upon mice, and the experiments will be found in detail in the article from which I have quoted.

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(13)

Traité Élémentaire de Chimie par Lavoisier. 3d. ed. 1801 t. ii, p. 178.

Expériences sur la Respiration des Animaux, et sur les Changemens qui arrivent à l' air en passant par leur poumon.

"I confined in a convenient apparatus, of which it will be difficult to give an idea without recourse to figures, 50 cubic inches of common air: I introduced into this apparatus 4 ounces of very pure mercury, and proceeded to the calcination of it by keeping up for twelve days a degree of heat almost equal to that which is necessary to make it boil.

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I observed that the air which the vessel contained was diminished by 8 or 9 cubic inches.

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This air thus diminished, would not precipitate lime water, but it extinguished flames, and caused animals placed in it to perish in a little while.

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In the preceding experiment, the mercury in calcining had absorbed the better part, the respirable part of the air, and had left the mephitic or non-respirable."

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By reduction he "re-established the air to almost exactly the state it had before calcination, that is to say, the state of common air. This air thus re-established, no longer extinguished flames, no longer killed animals which breathed it.

"Here then is an example of the very complete proof at which one can arrive by means of chemistry, the decomposition of the air and its recomposition; it evidently results:

First, that five-sixths of the air which we breathe is, as I have already announced in a preceding Memoir, in a mephitic state, that is to say, incapable of maintaining the respiration of animals, and the combustion of bodies.

Second, that the surplus, that is to say, one-sixth only of the volume of atmospheric air is respirable.

Third, that in the calcination of mercury, this metallic substance absorbs the healthful part of the air leaving only the mephitic.

Fourth, that in bringing these two parts of the air, thus separated, together, the respirable part and the mephitic part, one makes again air like that of the atmosphere."

(14)

An account of the Method of conveying Liquors immediately into the Mass of the Blood. By Mr. Oldenburg. Philosophical Transactions, London. No. 7, p. 128 (Abridgment, vol. i, p. 45).

"In this account it is asserted that the discovery of a method of conveying liquor immediately into the mass of the blood is due to Dr. Christopher Wren, at that time Savillian professor in the University of Oxford. The method which he followed was to make a ligature on the veins and having made an opening into them on the side of the ligature towards the heart, to introduce into them slender syringes or quills fastened to bladders (in the manner of clyster pipes) containing the matter to be injected; performing the operation upon pretty big and lean dogs, that the vessels might be large enough and

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easily accessible. These experiments were made at different times upon several dogs. Opium and the infusion of crocus metallorum were injected into the veins of the hind legs of these animals. The opium soon stupefied, though did not kill the dog; but a large dose of crocus metallorum induced vomiting and death in another dog. These experiments are more circumstantially related by Mr. Boyle, in his excellent book on the "Usefulness of Experimental Philosophy, Part II, Essay II, pp. 53–55."

A Letter from Dr. Timothy Clark. Phil. Trans. Lond. No. 35, p. 672,

Dr. Clark here gives the time of the infusing of liquors into the blood by Dr. Christopher Wren, showing that it was done in the house of the French ambassador, Duc de Bordeaux, in the year 1657.

In a letter from Dr. Timothy Clark in the Philosophical Transactions, No. 35,

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p. 672, it is stated that Dr. Richard Lower was the first who performed trans-

(16)

fusion on brutes, and that the French anatomist, Dr. Denis, was the first who tried it on man; that the account of Dr. Lower's experiment was published in the Phil. Trans. for Dec. 1666, but nothing was heard of Dr. Denis' operation until March 1667.

Richard Lower and Dr. King appear to have been the first who performed the experiment of transfusion of blood. The account will be found in *De Corde, item de motu et colore Sanguinis et Chyli in eo transitu*, 1669.

(17)

"The Method observed in Transfusing the Blood out of one Animal into another. By the Hon. Robert Boyle.

Phil. Trans. Lond. No. 20, p. 353. (Abridgment, 1809, vol. i, p. 128).

"The method here described was first practised by D. Lower of Oxford.

Take the carotid artery of the dog or other animal, whose blood is to be transfused into another of the same or a different kind, and separate it from the nerve of the eighth pair, and lay it bare above an inch. Then make a strong ligature on the upper part of the artery not to be untied again; but an inch below, viz: towards the heart, make another ligature of a running knot, which may be loosened or fastened as there shall be occasion. Having made these two knots, draw two threads under the artery between the two ligatures; and then open the artery and put in a quill, and tie the artery upon the quill very fast by those two threads, and stop the quill with a stick. After this make bare the jugular vein in the other dog about an inch and a half long; and at each end make a ligature with a running knot, and in the space betwixt the two running knots, draw under the vein two threads as in the other; then make an incision in the vein, and put into it two quills, one into the descendant part of the vein, to receive the blood from the other dog, and carry it to the heart; and the other quill put into the other part of the jugular vein, which comes from the head (out of which the second dog's own blood must run into the dishes).

"These two quills being put in and tied fast, stop them with a stick till there be occasion to open them. All things being thus prepared, tie the dogs on their sides towards one another so conveniently that the quills may go into

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each other, (for the dogs' necks cannot be brought so near, but that you must put two or three several quills more into the first two to convey the blood from one to another). After that unstop the quill that goes down into the first dog's jugular vein, and the other quill coming out of the other dog's artery; and by the help of two or three other quills put into each other, according as there shall be occasion, insert them into one another. Then slip the running knots and immediately the blood runs through the quills as through an artery, very impetuously. And immediately as the blood runs into the other dog, unstop the other quill coming out of the upper part of the jugular vein (a ligature being first made about his neck, or else his other jugular vein being compressed by one's finger); and let his own blood run out at the same time into dishes, (yet not too constantly, but according as you perceive him able to bear it) till the other dog begins to cry and faint, and fall into convulsions, and at last die by his side.

Then take out both quills out of the dog's jugular vein, and tie the running knot fast, and cut the vein asunder, (which you may do without any harm to the dog, one jugular vein being sufficient to convey all the blood from the head and upper parts, by reason of a large *anastomosis*, whereby both jugular veins meet about the *larynx*). This done, sew up the skin and dismiss him, and the dog will leap from the table and shake himself and run away, as if nothing ailed him."

(18)

Sir Charles Bell. Nervous System of the Human Body. Third Edition, London, 1844.

Page 24. "It was necessary to know, in the first place, whether the phenomena exhibited on injuring the separate roots of the spinal nerves corresponded with what was suggested by their anatomy. After refraining long, on account of the unpleasant nature of the operation, I at last opened the spinal canal of a rabbit, and cut the posterior roots of the nerves of the lower extremity; the creature still crawled, and there was no convulsion of the muscles of the back, but on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed."

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Page 25. "Every touch of the probe, or needle, on the threads of this root, was attended with a muscular motion as distinct as the motion produced by touching the keys of a harpsichord. These experiments satisfied me that the different roots, and different columns from which those roots arose, were appropriated to different offices, and that the notions derived from anatomy were correct."

Page 26. "On finding this confirmation of the opinion that the anterior column of the spinal marrow and the anterior roots of the spinal nerves were for motion, the inference presented itself that the posterior roots were for sensibility. But here a difficulty arose. An opinion prevailed that ganglions were intended to cut off sensation; and every one of those nerves, which I supposed were the instruments of sensation, have ganglions on their roots.

Some very decided experiment was necessary to overturn this dogma. I selected two nerves of the encephalon; the fifth which had a ganglion, and the seventh which had no ganglion. On cutting across the nerve of the fifth pair on the face of an ass, it was found that the sensibility of the parts to

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which it was distributed was entirely destroyed. On cutting across the nerve of the seventh pair on the side of the face of an ass, the sensibility was not in the slightest degree diminished.

By pursuing this inquiry, I found that the sole organ of sensation in the head and face is a ganglionic nerve. Ganglions were therefore no hindrance to sensation, but on the contrary, a necessary accompaniment to a nerve of sensibility; and thus my opinion was confirmed, that the ganglionic roots of the spinal nerves were the fasces or faciciculi for sensation.

Page 28. The nerve of the fifth pair was exposed at its root, in an ass, the moment the animal was killed; and on irritating the nerve, the muscles of the jaw acted, and closed with a snap. On dividing the root of the nerve in a living animal, the jaw fell relaxed. Thus its functions were no longer a matter of doubt: it was proved to be at once a muscular nerve and a nerve of sensibility. And thus the opinion was confirmed, that the fifth nerve is to the head what the spinal nerves are to the other parts of the body, in respect to sensation and volition."

(19)

Magendie. Expériences sur les fonctions des racines des nerfs rachidiens. Jour. de Phys. 1822, p. 276.

"A second, a third experiment gave me exactly the same result; I commenced to regard it as probable that the posterior roots of the spinal nerves could well have functions different from the anterior roots, and that they were more particularly destined for sensibility."

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p. 279. "I have repeated and varied these experiments upon several species of animals: the results which I am about to announce have been confirmed in the most complete manner, be it for the anterior members, or for the posterior. I shall pursue these researches and give a more detailed account of them in a future number. It is suficient for me to be able to announce to-day as positive that the anterior and the posterior roots of the nerves which take origin from the spinal cord have different functions, that the posterior seem more particularly detined for sensibility, while the anterior seem more especially connected with movement."

(20)

L'Œuvre de Claude Bernard. Paris. Baillière. Leçons de Physiologie; Substances Toxiques.

Système Nerveux. Recurrent sensibility, Vol. I. pp. 25–112.

Discovery of function of sympathetic in the neck, pp. 317–327.

Page 320. "However, the phenomena following seetion of the cervical branch of the great sympathetic are not limited only to the pupil. I have found that at the same time there is acceleration of the circulation in all the corresponding half of the head, the temperature of which rises; the skin becomes more sensitive; and, the arterial pulsation is stronger on this side, and the vessels are dilated."

For Bernard's work on the action of digitalis see

Action physiologique de la digitale et de la digitaline.

Gourvat, 4°, 74 pp., 1870.

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(21)

Brodie, B. C. Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons. Communicated by the Society for promoting the knowlege of Animal Chemistry. Read Feb. 21st, 1811. Philosophical Transactions, London, 1811, vol. xi, p. 178.

(22)

On the Reflex Function of the Medulla Oblongata and Medulla Spinalis. By Marshall Hall, M.D., F.R.S.L. and E. etc. etc. Read June 20, 1833. Philosophical Transactions 1833. p. 644.

"The first experiment which I made was upon the turtle. The animal was decapitated in the manner usual with cooks, by means of a knife, which divided the second or third vertebra.

"The head being placed upon the table for observation, it was first remarked that the mouth opened and shut, and that the submaxillary integument descended and ascended, alternately, from time to time, replacing the acts of respiration. I now touched the eye or eye-lid with the probe. It was immediately closed: the other eye closed simultaneously. I then touched the nostril with the probe. The mouth was immediately opened widely, and the submaxillary membrane distended. This effect was especially induced on touching the nasal fringes situated just within the anterior part of the maxilla. I passed the probe up the trachea and touched the larynx. This was immediately followed by a forcible convulsive contraction of the muscles annexed to it. Having made and repeated these observations, I gently withdrew the medulla and brain. All the phenomena ceased from that moment. The eye, the nostril, and larynx were stimulated, but no movement followed.

"The next observations were made upon the other parts of the animal. The limbs, the tail, were stimulated by a pointed instrument or a lighted taper. They were immediately moved with rapidity. The sphincter was perfectly circular and closed; it was contracted still more forcibly on the application of the stimulus. The limbs and tail possessed a certain degree of firmness or tone, recoiled on being drawn from their position, and moved with energy on the application of a stimulus. On withdrawing the spinal marrow gently out of its canal, all these phenomena ceased. The limbs were no longer obedient to stimuli, and became perfectly flaccid, having lost all their resilience. The sphincter lost its circular form and its contracted state, becoming lax, flaccid, and shapeless. The tail was flaccid, and unmoved on the application of stimuli.

"These experiments afford evidence of many important facts in physiology. It proves that the presence of the medulla oblongata and spinalis is necessary to the contractile function of the eyelids, the submaxillary textures, the larynx, the sphincters, the limbs, the tail, on the application of the stimuli to the cutaneous surfaces of mucus membranes. It proves the reflex character of this property of the medulla oblongata and spinalis, and the dependence of these motions upon the reflex function. It proves that the tone of the limbs, and the contractile property of the sphincter, depend upon the same reflex function of the medulla spinalis—effects not hitherto suspected by physiologists.

"On another occasion, having removed the head of a frog, I divided the spine between the 3rd and 4th vertebra, and separated the upper portion of the animal from the lower. There were then the head, the anterior extremity,

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and posterior extremity, with their corresponding portions of medulla, as three distinct parts of the animal. Each preserved the reflex function. On touching the eye, it was retracted, and the eyelids closed, whilst similar phenomena were observed simultaneously in the other eye. On removing the medulla, these phenomena ceased. On touching the toe of one of the anterior extremities, the limb and the opposite limb equally moved. On removing the spinal marrow, this phenomenon also ceased. Precisely similar efforts were observed in regard to the posterior extremity.

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"One of the most remarkable of the phenomena attached to the reflex function in animals, is that presented by those muscles of the hedgehog (Erinaceus Europaeus) by means of which that animal assumes, in certain circumstances, the form and firmness of a ball. The reflex function seems specially to connect the roots of the spine with the muscles. If the animal be examined under the influence of hybernation, the reflex function continues for some hours after the brain has been removed: the panniculus carnosus, the limbs, the tail, the larynx, the sphincter ani, remain excitable, and retain a degree of tone. These phenomena cease on removing the medulla spinalis.

"In the case of the decapitated young hedgehog, after all gasping had ceased, motions of the larynx are still excited on irritating the nostrils, or on irritating the medulla itself; just as the peculiar motions of the trunk are excited on irritating the limbs, tail or spines,—or the spinal marrow.

"Nor are we without evidence that the same principles obtain in the human subject. The condition of the infant born without cerebrum or cerebellum, and breathing from the influence of the medulla oblongata alone, is precisely that of the reflex function, with the addition of respiration. Such a case has been witnessed and described by Lawrence. 'The child moved briskly at first, but remained quiet afterwards, except when the tumor was pressed, which occasioned general convulsions. It breathed naturally, and was not observed to be deficient in warmth, until its powers declined. I regret that from fear of alarming the mother, no attempt was made to see whether it would take the breast: a little food was given it by the hand. It voided urine twice the first day, and once a day afterwards; it had three dark colored evacuations. The medulla spinalis was continued for about an inch above the foramen magnum, swelling out into a small bulb, which formed the soft tumor on the base of the skull. All the nerves from the fifth to the ninth were connected to this.' This brief detail is full of interest. The respiration was natural, the medulla oblongata being entire. Swallowing was affected when food was brought into contact with the pharynx; the sphincters performed their functions; the limbs were moved when the skin was first impressed by atmospheric air. There was no indication of sensation—the child remained quiet after the first brisk movements; and no event is mentioned which could establish the existence of voluntary motion,—the acts of swallowing, and of the expulsion of the urine and faeces, with the functions of the larynx and of the sphincters, belong distinctly to the excito-motory system."

(23)

Œuvres de Legallois avec des notes de M. Pariset. Paris 1824, t. I, p. 64.

"Respiration does not depend upon the whole brain, but upon a quite circumscribed part of the medulla oblongata, which is situated at a little dis-

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tance from the occipital foramen and toward the origin of the nerves of the eighth pair (or pneumogastrics). For if one opens the cranium of a young rabbit, and extracts the brain by successive portions, from before backward, by cutting slices, one can remove in this manner all of the brain so-called, and afterward all of the cerebellum and a portion of the medulla oblongata. But it (respiration) ceases suddenly when one comes to include in the section the origin of the nerves of the eighth pair."

(24)

Recherches Expérimentales sur les Propriétés et les Functions du Système Nerveux dans les Animaux Vertébrés par P. Flourens. Paris 1842, p. 55.

§ VI. "General Conclusions of the Chapter.

I. The results obtained upon reptiles and mammals reproduce then and confirm the results given by birds:

With destruction of the cerebral lobes coincides constantly loss of volition and perception;

With destruction of a single lobe, loss of vision in the opposite eye;

With the destruction of the cerebellum, loss [of the power] of jumping, flight, walking, standing, etc.;

With destruction of the medulla oblongata, of the spinal cord, of the nerves, [coincides] loss of muscular contraction, and, in consequence, loss of movement, and death.

II. Contractions, the immediate excitation of contractions, the association of these contractions in movements of the whole body, the coördination of these movements in jumping, flying, walking, or standing, etc., the willing of these movements, sensations, perceptions, all these phenomena are then independent; the organs from which they are derived, distinct; their isolation, manifest; their localization, demonstrated."

§ II.

Page 189. I.—We have seen in the preceding chapter that the medulla oblongata is, in all these classes [mammals, birds, frogs, reptiles, and fishes], the organ which is the prime mover or the chief *exciter* and *regulator* of the inspiratory movements; it is, moreover, in all these classes, the organ immediately productive, through the nerves, of the inspiratory movements, particularly of the face and head; lastly, it is, in fine, in fishes, as I shall show, the prime moving organ, and the organ immediately productive of *all* respiratory movements.

II.—The medulla oblongata is then, in all the classes, the essential and primordial organ of the respiratory mechanism; it is the exclusive organ of this mechanism in fishes."

(25)

An Account of Mr. Hunter's Method of Performing the Operation for the Cure of Popliteal Aneurism. Sir Everard Home, Bart. Works of John Hunter with Notes by J. F. Palmer, 4 vols. London, 1835. Vol. iii, p. 596.

"Mr. Hunter finding an alteration of structure in the coats of the artery previous to its dilation, and that the artery immediately above the sac seldom unites when tied up in the operation for aneurism, so that as soon as the ligature comes away, the secondary bleeding destroys the patient, was led to conclude that a previous disease took place in the coats of the artery, in conse-

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quence of which it admitted of dilatation capable of producing an aneurism. But not satisfied with the experiments on frogs, given by Haller in support of the opinion that weakness alone was sufficient to produce the dilatation, he resolved to try the result in a quadruped, which, from the vessels being very similar in structure to those of the human subject, would be more likely to ascertain the truth or fallacy of Haller's opinion."

Mr. Hunter's account of the experiment: *Ibid.*, vol. i, p. 544.

"However, whatever may have been either the remote or immediate cause," [of aneurism] "it must, in fact, in all cases arise from a disproportion between the force of the blood and the strength of the artery, the coats being weakened so as not to be able to support the force of the blood in its passage along its canal, which therefore gives way. This weakness of the coats of the artery would appear, in most cases, to depend on disease, for accidents, *cæteris paribus*, have generally the power of recovery. As a proof of this, I will relate an experiment made to ascertain the truth of the existence of the mixed kind" [of aneurism], "which was supposed to arise from a partial destruction of the coats of an artery, and that the remaining coat being too weak to sustain the force of the circulation, gave way and distended. That the artery might have the full force of the blood's motion, I chose the carotid, as being near the heart.

One of the carotid arteries of a dog, for an inch in length, was laid bare, and its coat removed, layer after layer, until the blood was seen through the remaining transparent coat, and I had gone as far as I dared; I then left the artery alone for three weeks, when I killed the dog, expecting to find a dilatation of the artery as had been asserted; but to my surprise the sides of the wound had closed on the artery, and the whole was consolidated to and over it, forming a strong bond of union, so that the whole was stronger than ever."

Vol. III, p. 598. "Mr. Hunter, from having made these observations was led to propose that in this operation" [for popliteal aneurism] "the artery should be taken up in the anterior part of the thigh, at some distance from the diseased part, so as to diminish the risk of hemorrhage, and admit of the artery being more readily secured, should any such accident happen. The force of the circulation being thus taken off from the aneurismal sac, the progress of the disease would be stopped; and he thought it probable, that if the parts were left to themselves, the sac, with its contents, might be absorbed, and the whole of the tumor removed, which would render any opening into the sac unnecessary."

Experiments and Observations on the Growth of Bones, from the Papers of the Late Mr. Hunter. (Published by Mr. (afterwards Sir Everard) Home, in the Second Volume of the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge.) *Ibid.*, vol. iv, p. 315. Read October 4th, 1798.

"It was some time anterior to the year 1772 that Mr. Hunter began to investigate this subject, and an account of the experiments and observations was given to me to copy in that year, as a part of his future lectures.

Du Hamel had published a very ingenious theory upon the growth of bones, which he endeavored to support by experiments tending to prove that bones grow by the extension of their parts. With this doctrine Mr. Hunter was not satisfied, and instituted experiments to determine the truth of Du Hamel's opinion.

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Mr. Hunter began his experiments by feeding animals with madder, which has the property of tinging with a red color that part only of the bone which is added while the animal is confined to this particular food. He fed two pigs with madder for a fortnight, and at the end of that period one of them was killed; the bones, upon examination externally, had a red appearance; when sections were made of them, the exterior part was found to be principally colored, and the interior was much less tinged.

The other pig was allowed to live for a fortnight longer, but had no madder in its food; it was then killed, and the exterior part of the bone was found of the natural color, but the interior was red.

He made many other experiments of the same kind upon the increase of the thickness of the neck and head of the thigh bone. From thence it appeared that the addition of new matter was made to the upper surface, and a proportional quantity of the old removed from the lower, so as to keep the neck of the same form, and relatively in its place.

To ascertain that the cylindrical bones are not elongated, by new matter being interposed in the interstices of the old, he made the following experiment: he bored two holes in the tibia of a pig, one near the upper end, and the other near the lower; the space between the holes was exactly two inches: a small leaden shot was inserted into each hole. When the bone had been increased in its length by the growth of the animal, the pig was killed, and the space within the two shot was also exactly two inches.

This experiment was repeated several times on different pigs, but the space between the two shot was never increased during the growth of the bone.

Besides these experiments on the growth of bones, he made others, to determine the process of their exfoliation.

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Bones, according to Mr. Hunter's doctrine, grow by two processes going on at the same time, and assisting each other; the arteries bring the supplies to the bone for its increase; the absorbents are at the same time employed in removing portions of the old bone, so as to give to the new the proper form. By these means the bone becomes larger, without having any material change produced in its external shape."

(26)

Experiments and Observations on the Union of Fractured Bones. By John Howship, Esq. Read March 17th, 1817. Medico-Chirurgical Transactions. London, 1818, vol, ix, p. 143.

Page 145. "The following experiments were made upon rabbits, selected at about the age of twelve months, the period at which, from their beginning to bear young, they may be considered to have nearly attained their full growth."

Six experiments were performed.

Page 170. "Having at length completed the account of my observations upon fracture, I shall now lay before the Society the conclusions drawn from the above enquiry, which will close the present paper.

The first effect of fracture is extravasation of blood into the surrounding soft parts, the quantity poured out varying according to the degree of contusion or complication."

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Page 171. "The blood effused in fracture suffers various degrees of change; but under all circumstances it forms the medium in which the ossific process is established."

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Page 172. "The mode of progress in the ossific process seems to indicate a degree of caution, as if a principal object was to guard against the possibility of the least disturbance or motion between the parts of the bone, subsequent to the act of union."

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"The circumstances of the fracture evidently regulate the quantity and seats of the ossific deposit. In simple transverse fracture with little contusion, where the bone is immediately reduced, and the limb kept perfectly quiet, the degree of internal laceration will be small, the effusion of blood inconsiderable, and the ultimate deposit of bone moderate in proportion."

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Page 178. "In oblique fracture, where the bones have suffered more violence at the moment of accident, and are retained with more difficulty when reduced, the effusion of blood will be greater, and the quantity of ossific matter formed will be also more abundant."

(27)

Flourens P. Récherches sur la formation des os. Compt. rend Acad. d. sc., Paris, 1844, xix, 621–625.

(28)

Heine, B. Ueber die Wiedererzeugung neuer Knochenmasse, und Bildung neuer Knochen. J. d. chir. u. Augenh. Berlin, 1836, xxiv, 513–527, also Gaz. Méd. de Paris, 1837, v, 386–388.

(29)

Syme. Trans. Roy. Soc. Edin., 1836, vol. xiv, p, 158.

(30)

Ollier, L. Des moyens chirurgicaux de favoriser la reproduction des os après les résections ; de la conservation du périoste ; résections sous-périostées ; de la conservation de la couche osseuse périphirique ; evidement des os.

Gaz. hebdom. de méd. Paris, 1858, v. 572, 651, 733, 769, 853, 899.

Ollier, L. Recherches experimentales sur la production artificielle des os, ou moyen de la transplantation due périoste et sur la régénération des os, aprés les résections et les ablations completes.

J. de la physiol. de l'homme, Par. 1859, ii, 1169, 468.

Ollier, L. De la transplantation des éléments anatomiques du blastème sous- périosteal ; formation des petites grains osseux dans la region où ont été semés ces éléments.

Compt. rend. Soc. de biol., Par. 1860, 3 s., i, 108.

Ollier, L. Nouvelle démonstration de la régénération osseuse après les résections sous- périostées articulaires.

Bull. gen. de thérap. etc., Paris, 1870, lxxix, 258–261.

Du Périoste au Point de Vue Physiologique et Chirurgical, communication faite au congrès medical de Lyon le 28 September, 1864, par M. Ollier, chirur-

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gien en chef de l'Hôtel—Dieu de Lyon. Gaz. hebdom. de mèd. Par. 1865, 2 s. ii, 82, 116, 152, 195.

"Proposition First. That the periosteum produces osseous tissue by a normal development, in the order of its proper anatomic elements. The deeper layer, composed of protoplasmic cells, possesses this property, and to this layer I have given the name osteogenic."

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"I first repeated the experiments of my predecessors, but in studying the role of the periosteum, with Du Hamel in fracture, or Heine and M. Flourens in resections, I have recognized that it was difficult to determine the part played by the divers elements of the bone in the act of reproduction."

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"I isolated the different tissues, I studied them separately, either in their normal situation, preserving the while their anatomic relations, or displacing and transplanting them into distant regions. I experimented with the periosteum, the marrow, cartilage, bone, and the adjacent tissues, muscles and tendons and I arrived at results which permit the setting forth of propositions which I believe sufficiently exact to prevent all controversy."

"I commenced with the periosteum, which I detached from the bone; I first dissected up a piece of this membrane, 5 or 6 cm. long, from the tibia of a rabbit, I rolled it around the limb amongst the muscles and under the skin, and I obtained bone, or rather, osseous prolongations of varied form. I produced bone in a circle, in a spiral, in a cross, etc. etc.; and finally I gave to the new bone any form I desired, and for this purpose I had but to fix the periosteum in a predetermined way: after from 20–25 days (in the rabbit, the cat, or the dog) I found new bone of the form of the periosteum, or to speak more correctly, I found the periosteum ossified.

This experiment semed to me fundamental, it furnished simple and irrefutable proof of the osteogenic property of the periosteum; and it answered the greater part of the objections which had been offered to the doctrine of Du Hamel, from the time of Haller to Bichat.

It proved that the periosteum produces bone of itself, independently of the neighboring tissues; and from a surgical point of view it promised new resources in autoplasty; it also showed us the manner in which ossification takes place in abnormal regions. But I did not stop at this first experiment which I saw was so signal. Being anxious to obtain results of surgical value, I modified it so as to make it still more convincing, and by means of it answered all objections that it was possible for me to foresee.

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After having detached and fixed my shred of periosteum among the muscles, I left it to live, or at least to form certain adhesions during three or four days; then, finding that it had become ossified, I detached from the bone 4 or 5 mm. of the entire depth of the periosteum, in a manner to interrupt all connection between the periosteum and bone. I established then that, in spite of this interruption, the periosteum continued to ossify, and that new bone, independently of the normal bone, was formed there.

But this did not yet satisfy me. To answer at once all possible objections, I conceived the idea of transplanting the periosteum into distant regions, immediately after its separation from the bone.

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I transplanted it from the leg to the forehead or back, and I saw that this membrane carried with it everywhere its osteogenic property. Everywhere I engrafted the periosteum new bone was formed; this was not an unformed mass of calcareous particles, but a bone formed of the characteristic elements of osseous tissue, hollowing itself out into spaces in its interior, and having after a certain time a veritable canal containing medullary substance, and surrounded by a compact layer."

(31)

Mr. Rand. "A new method for the treatment of neuralgia by sub-cutaneous injection, 1855.

(32)

John Hunter, *loc cit.*

(33)

A Treatise on the Process Employed by Nature in Suppressing Hemorrhage from Divided and Punctured Arteries. J. F. D. Jones. 8°, Lond. 1802.

I have been unable to find the above treatise, but the following account of the experiments is given by Travers, *loc cit.* p. 440.

"Jones ascertained, that the effusion of lymph from the wound inflicted by the ligature was sufficient, even if the ligature were removed upon the instant, to obstruct the artery. By including a loose thread along with the artery in the ligature, he readily withdrew the latter after the infliction of the wound. In one instance he succeeded with a single ligature, and in several instances with two, three or four, made at a small distance apart. The lymph effused was in proportion to the extent of the section, or if this was incomplete, the union was equally so. He was led to conclude that the complete circular section of the internal coat was indispensable to union, and the success which attended his experiments led him to conjecture, that in some surgical cases removing the ligature as soon as it was made would be an efficient operation. This suggestion, the value of which he left to be determined by future experiments was caught at with eagerness by his readers, and by many considered to be the essence of his publications."

In a foot note, the following quotation is given from Jones, p. 136.

"I leave the the fact (viz.—the complete obstruction of an artery consequent upon the momentary application of a ligature) for those who have opportunities of applying it in practice, when all the circumstances which determine its success or failure shall have been fully ascertained by further experiments on brutes."

(34)

Observations upon the Ligature of Arteries and the Causes of Secondary Hemorrhage. Benjamin Travers. Medico-Chirurgical Transactions, vol. iv, p. 435. Read October 26th, 1813.

Page 439. "It is curious to observe the revolution which has taken place within a few years in this branch of surgical practice, since experimental inquiry has furnished the true explanation of the principle upon which the ligature acts. Mr. Hunter and the surgeons who after him practised the operation for popliteal aneurism, were in the habit of applying the ligature with force only sufficient to bring the sides of the vessel in contact; and some

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included an extraneous body, as a piece of cork or wood, or a roll of linen, to prevent the lesion of the artery in the act of tightening the ligature. The fear of cutting the coats of the artery was uppermost in the minds of all, and next to this, the fear of quickening the process of ulceration, and the casting off of the ligature."

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Page 443. "The original experiment of Jones, in whatever light we view it, is of unquestionable importance, and deserves to be highly appreciated. While its occasional failure demonstrates that the apposition of the cut surfaces is essential to the certain obliteration of the vessel, its occasional success establishes that, *coeteris paribus*, it cannot with this precaution fail of its intention." As a basis for the statements made in this paper, Travers performed five experiments upon the ligaturing of arteries, using the ass, dog, and horse.

In another paper on the same subject, which appears in vol. vi of the Transactions of the Medico-Chirurgical Society, p. 632, he records nineteen other experiments.

The first eight were undertaken "To ascertain the earliest period at which the ligature might be removed, and the artery wounded without hemorrhage."

Page 643. "The experiments next to be related, give the operation of the compressor, and were undertaken with a view to determine its merits as a surgical instrument, comparatively with the ligature. Professor Assalini of Milan, who lately visited this country, entertains a preference for the practice of compression in the operation for aneurism. He had employed it with success in three cases of popliteal aneurism."

Experiment XV.—"I wished to know the effect of leaving the compressor upon the vessel, and the time in which it was liberated by ulceration."

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Page 658. "In contemplating the removal of the ligature at a given time, it becomes essential to ascertain if this can be done with equal security when a branch is contiguous as when at a distance. With this view I made the following experiments." (Exp. XVI–XIX.)

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Page 662. "The practical application of the facts and deductions contained in this and my former Essay (Vol. IV) will probably be the subject of a future communication to the Society.

"It is however, in my judgment, a subject too important to be lightly disposed of; and it carries with it, in reference to surgical practice, a responsibility too serious to justify a rude and hurried trial of its merits."

(35)

On the Torsion of Arteries as a means of Arresting Hemorrhage, with Experiments by Thomas Bryant, F.R.C.S. Medico-Chirurgical Transactions, vol. li, p. 199 (1868).

Page 203. "I propose to relate seriatim the experiments I have made upon the dog, horse and human subject to test the value of torsion, and to observe the process by which the vessels so treated become permanently sealed."

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"Experiment 1. February 4th, 1868.—I divided the left *femoral artery* of a dog just below Poupart's ligament, and twisted the cardiac end by 'free' tor-

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sion *four* times, with success. During this time the distal end was held by forceps, and when these were removed hemorrhage occurred; the bleeding extremity was, however, seized by forceps and twisted *four* complete revolutions, all bleeding at once ceased, and by the seventh day the wound had united.

The dog was killed the 11th day after the operation."

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"Experiment IV.—February 11th, 1868.—I cut down upon and divided the right *common carotid* artery of a dog. I applied 'free' torsion to the cardiac end, making three revolutions without success, and accordingly seized the vessel again and twisted it four times more. Hemorrhage was at once arrested. *Three* complete twists were then given to the distal end of the artery, and no bleeding followed.

On the second day the dog was quite well, he had taken his food as usual, and appeared in no way disturbed by the operation. On the following day the animal was destroyed.

It must be noticed that in this case, as in the second experiment, three rotations of the artery were not sufficient to arrest bleeding; four proved successful in both cases."

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"Experiment VII. March 17th, 1868.—I cut down upon and divided the left *common carotid* artery of a horse; applied two pairs of torsion forceps transversely to the vessel, and divided the artery midway between them, leaving an inch of artery on the distal side of each pair of forceps. With a third pair of torsion forceps, I then seized the extremity of the artery at its cardiac end, and twisted it *seven* complete revolutions. I then removed the instrument that fixed the vessel, and not a drop of blood escaped; the pulsations in the vessel were very strong. The same treatment was then applied to the distal end with a like result. It was certainly something astonishing to see the great vessel fill out and pulsate after the operation without one drop of blood escaping; and although the animal plunged somewhat during and after the operation, the success was must complete. The animal was allowed to live for forty-eight hours, and then killed."

(36)

Rayer and Davaine. Bull. de la Soc. de Biol. de Paris, 1850.

"In the blood are found little thread-like bodies about twice the length of a blood corpuscle. Those little bodies exhibit no spontaneous motion." However, no importance was attached to their presence.

Davaine. Nouvelles recherches sur les infusiores du sang dans la maladie connue sous le nom de sang de rate. Compt. rend. Soc. de Biol. 1863. Par. 1864, 3. s. v, 149–152.

(37)

The Life of the Trichina. (Monograph) 1864, p. 21. By Rudolph Virchow, M.D., Ph.D. Translated by Rufus King Brown, M.D.

The author states that he received from Dr. Zenker some of the muscle of a girl who died of trichinosis and also some of the flesh of the pig that caused her disease.

"A rabbit fed with the trichina from the girl, died in a month with its

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flesh full of them. Some of its flesh was given to a second rabbit. It also died in a month. With this meat three other rabbits were fed. Of these, two died at the end of the third week, and the other in the fourth week. To another animal the meat of this was fed. As it ate but little it lived six weeks. In all these the muscles after death were found filled with trichina. Even in the smallest particle of their meat several were found."

(38)

Cause and Nature of Tuberculosis. J. A. Villemin. Gaz. hebdom. de mèd. Par. 1865, 2. s., ii, 795.

"1. Tuberculosis is the effect of a specific agent, of a virus, in a word.

2. This agent should be found, as its congeners, in the morbid products which it gives rise to by a direct action upon the normal elements of the tissues affected.

3. Introduced into an organism susceptible to it, this agent should then reproduce itself, and reproduce, at the same time, the disease of which it is the essential and determining cause.

Experimentation is undertaken to confirm these inductive conclusions. The results are as follows:"—

1st Series. Of two rabbits, one is inoculated with two little fragments of tubercular tissue and pus from a lung cavity. The other is kept as a control. The two were placed under like conditions of existence. At the autopsies the inoculated rabbit was found to be infected, while its mate showed absolutely no sign of tubercle.

2d Series. Four rabbits were inoculated with tubercular matter, and, when killed, all of them were found infected. Two other rabbits which had been kept with these and afterward used for physiologic purposes presented no trace of tuberculization.

From these experiments, Villemin draws the following conclusions:—

"1. Pulmonary phthisis (as tubercular diseases in general) is a specific infection.

2. Its cause is an inoculable agent.

3. The inoculation can easily be made from man to the rabbit.

4. Tuberculosis belongs then to the class of virulent diseases, and should have a place, in the nosologic table, along with syphilis, but better, perhaps, with glanders and farcy."

Continuing his experiments to the 3d Series, three pairs of rabbits are taken, and one of each pair inoculated. Two of the pairs are put in the same cage. At the autopsies those not inoculated show no signs of tuberculosis.

(39)

De l'atténuation du virus du choléra des poules par M. L. Pasteur. Comptes Rendus, t. xci, p. 673.

Chamberland. Le Charbon et la Vaccination Charbonneuse d'après les travaux recents de M. Pasteur. (Paris, Tignol 1883), p. 9 *et seq.*

Translation for New Sydenham Society, 1886, p. 551 *et seq.*

"The experiments were commenced in the early days of August, 1878. They consisted at first in feeding certain lots of sheep with lucerne which had been watered with artificial cultivations of the bacterium of anthrax full of the parasite and its spores. * * * * Notwithanding the immense

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number of the spores of the bacterium swallowed by all the sheep of each lot many of them, often after having been distinctly ill, escaped death; a smaller number died with all the symptoms of spontaneous anthrax after a period of incubation which might extend to eight or ten days, although, at the end, the disease took on the almost sudden characters frequently noted by observers who have thus been led to believe in a very short period of incubation.

The mortality was increased by mixing with the food, sprinkled with the spores, sharp-pointed objects, especially the pointed extremities of the leaves of dried thistles, and above all the beards of ears of barley cut into small fragments about a millimeter long.

It was of great importance to ascertain whether the autopsy of animals dying under these conditions would show similar lesions to those observed in animals dying spontaneously in stables, or in flocks penned in the open air. The lesions in the two cases are identical, and their nature authorized the conclusion that the disease begins in the mouth or pharynx."

De l'atténuation des virus et de leur retour à la virulence, par M. L. Pasteur avec la collaboration de MM. Chamberland et Roux. Comptes Rendus, t. xcii. p. 429.

"I have made known in papers recently published the first example of the attention of a virus by experimental means alone. * * * * It seems probable that the oxygen of the air is the chief cause of these attenuations, that is to say, of these diminutions in the facility with which the microbe multiplies; for it is clear that the various degrees of virulence are identified with the varying power of the parasite to develop in the economy. * * * * The virus of anthrax, being one of the best studied, must be the first to attract our attention. A mycelial growth of the bacterium entirely free from spores can be maintained in contact with pure air at a temperature between 42° C. and 43° C.

After an interval of about one month the cultivation is found to be dead, that is to say, fresh broth inoculated with it remains completely sterile. On the day before that on which this inability to grow is noted, and on every preceding day during the month, reproduction of the growth is, on the contrary, easy.

With regard to its virulence we discover this remarkable fact: after remaining for eight days at a temperature of 42° to 43° C., and ever afterwards the bacterium has lost its virulence; at least its cultivations are inocuous to the guinea-pig, the rabbit, and the sheep, three of the animals most likely to contract splenic fever. We are, therefore, by using a simple artifice in cultivating, able to produce not merely an attenuation of virulence, but a suppression which is apparently complete. More than this, we have the power of preserving and cultivating the terrible microbe in this inoffensive condition.

Experimental application of the Method" [of inducing immunity].

"M. Pasteur proposed that 60 sheep should be used for this experiment, and consented, at the request of the President of the Agricultural Society, to extend the experiment to 10 cows. He foretold that all sheep not protected by inoculation of attenuated virus would die, and that all the cows not so protected would be, at least, made ill, and that some would die when inoculated with a very virulent virus; while all the protected sheep would survive the inoculation with this very virulent virus, and that the cows would not be

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made ill; 10 sheep were not to be dealt with in any way, but kept for ultimate comparison with the inoculated sheep."

For further work done by Pasteur on immunity consult

Sur la rage, par M. Pasteur avec la collaboration de MM. Chamberland et Roux. Comptes Rendus, t. xcviii, p. 1229, and

Méthode pour prevenir la rage après morsure, par M. L. Pasteur. Ibid., t. ci, p. 766.

(40)

The Etiology of Tuberculosis, by Dr. Robert Koch. Translated by Mr. Stanley Boyd in Microparasites in Disease, pp. 157–160. Infection Experiments with Tissue containing Tubercle Bacilli.

"The inoculation was effected by making a small incision in the abdominal wall of a guinea-pig with the scissors, inserting the point of the scissors to form a pocket-like subcutaneous wound about a half centimeter deep. Into this little pocket a fragment of the inoculation substance about the size of a millet or mustard seed was pushed as deeply as possible. On the following day the inoculation wound was always united, glued together and showed no reaction. Generally it was not till after a couple of weeks that a visible swelling of the lymphatic glands next the seat of inoculation occurred, usually the inguinal glands on one side, and at the same time induration and the development of a nodule took place in the inoculated wound, which up till then had remained perfectly healed. After this the lymphatic glands enlarged rapidly, frequently to the size of a hazel-nut, the nodule at the seat of inoculation then generally broke and became covered with a dry crust, beneath which was a flat ulcer with a cheesy floor, discharging very slightly. The animals began to lose flesh about this time, their coat became bristly, dyspnœa set in, and they died generally between the fourth and eighth weeks, or they were killed within the same space of time. In some instances the inoculation substance was inserted into a pocket-like wound in the skin of a rabbit also. But as the course of the disease was not so constant and rapid as in the guinea-pigs after subcutaneous inoculation, I inoculated rabbits afterwards only in the anterior chamber of the eye.

The following inoculations were carried out in the way above described:—

1. Miliary tuberculosis. Tubercle of the pia mater, very rich in tubercle bacilli; 6 guina-pigs. Of these one died 5, two 6, and two 7 weeks after inoculation. The sixth was killed in the eighth week. In all the animals the lungs, liver and spleen were highly tubercular, and the inguinal glands had undergone caseation.

2. Miliary tuberculosis. Grey nodules in the lungs, with fairly numerous tubercle bacilli: 6 guinea-pigs. Three died in the sixth week; the rest were killed some days later. All tubercular, as in No. 1.

3. Miliary tuberculosis. Grayish yellow nodules from the spleen and kidneys, with not many tubercle bacilli: 6 guinea-pigs. Died in the 6th and 7th weeks. All tubercular, as in No. 1.

4. Miliary tuberculosis. Grey nodules from the lung, fairly rich in bacilli: 3 guinea-pigs. Two died in the 6th, one in the 7th week. All tubercular, as in No. 1.

5. Miliary tuberculosis. Grey nodules from the lung containing few bacilli: 5 guinea-pigs, 2 rabbits at the root of the ear. One guinea-pig died after 8

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weeks, the remainder were killed some days later. All were tubercular. The rabbits killed after 10 weeks had caseous lymphatic glands at the root of the ear and in the neck, tolerably abundant grey nodules in the lungs, a few in the kidneys and the spleen. Five more guinea-pigs were inoculated with the tubercles from the spleen of one of the guinea-pigs. Three of these died in the 8th week. The two remaining were killed the same week, and all found tubercular. Some of the cheesy glandular substance from a rabbit was rubbed up with water and injected into the peritoneal cavity in two rabbits. When these two animals were killed after 8 weeks, tuberculosis of the omentum, spleen and liver was found, together with a fair number of gray nodules in both lungs.

6. Caseous pneumonia and tuberculosis of the meninges: 2 guinea-pigs inoculated with the cheesy substance from the lungs in which there were numbers of bacilli. The animals died in the 5th and 6th weeks. All tubercular.

7. Lungs showing caseous infiltration with many bacilli: 6 guina-pigs. The first died after 6 weeks, The remainder were very ill at the time and were killed a few days later. All tubercular.

8. Phthisical lungs with cavities, intestinal ulcers and cheesy mesenteric glands. Two guinea-pigs were inoculated from the contents of cavity containing a fair number of bacilli, and four more from the mesenteric glands, which were very full of bacilli. The latter died in the 5th and 6th weeks: of the first two, one died in the sixth week, and the other was killed a few days later. All tubercular.

9. Caseous bronchitis and intestinal tuberculosis. Five guinea-pigs were inoculated from the lung substance, in which there was a good number of bacilli. Two of them died in the 8th week. The remainder were killed before the end of the same week. All tubercular.

10. Phthisical lungs with cavities. Four guinea-pigs inoculated from the consolidated lung tissue, in which were only a few bacilli. Three of them died in the 7th and 8th weeks, the last not till the 12th week. All tubercular.

11. Phthisical sputum. Nine guinea-pigs were inoculated at different times with fresh sputum containing a varying number of tubercle bacilli taken from three different patients. Some of the animals died before the 8th week, some were then killed. They were all tubercular.

12. Phthisical sputum dried for 2 weeks: 3 guinea-pigs. Two died in the 6th week, the third was killed at the same time. All tubercular.

13. Phthisical sputum dried for two months: 3 guinea-pigs, killed after 5 weeks, and tubercles found in lungs, liver, and spleen.

14. Tuberculosis of the uterus and tubes. Six guinea-pigs inoculated with cheesy material from the tubes. Two animals died at 7 weeks. The others were killed in the 9th week. All tubercular.

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28. With lung tubercles from a second monkey, dying of spontaneous tuberculosis, 2 guinea-pigs were inoculated and died of tuberculosis in the 8th and 9th weeks. From these guinea-pigs again 2 guinea-pigs and 1 rabbit were inoculated. They were killed in the 6th week, as they seemed already ill, and they were found to be already tubercular.

Two more guinea-pigs were inoculated from the same monkey with lung tubercles which had been dried and kept for 3 days. They too were killed in the 6th week, and found tubercular.

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For the infection experiments just detailed (including 18 not quoted) 79 guinea-pigs, 35 rabbits and 4 cats were used altogether, and the inoculation of these animals resulted in tuberculosis without exception."

(41 and 42)

Experiments on the Immunity and Cure of Tetanus in Animals. Zeitschr. f. Hygiene u. infections-Krankheiten, 12, 1892, 45–57. By Dr. Behring.

"In November, 1890, I, in an announcement with Mr. Kitasato, stated that with the blood of a rabbit rendered immune from tetanus, we could prevent mice from taking the disease, and if they had been infected we could cure them.

The certainty of the cure and the immunity of even such animals as had received more than a hundred times the dose of the fatal infection, exceeded our greatest expectation. If in the manipulation no technical mistakes were made, unfavorable results were entirely excluded."

Here the author states that the practicality of this method was suggested by his experiments in diphtheria, and that he and Dr. Kitasato arranged to do all they could to perfect the new method so it could be used for larger animals than mice, and especially that it might be used to render the human body immune from tetanus. They were stimulated to their research by the belief that they had a method which was applicable to different infectious diseases.

Drs. Kitasato and Behring carried on their investigations separately, but each assisted the other where possible.

The successful use of ICl_3 in diphtheria led Dr. Behring to apply it for the purpose of rendering immune from tetanus.

The author states that the experiments upon rabbits were very successful and that the experiments were among the easier tasks which a bacteriologist had to perform. The first requisite of success being an exact knowledge of the action of the culture relatively to the filtrate. In one month Dr. Behring received 8 cultures from Dr. Kitasato and tested the effectiveness of them on mice and rabbits. He gives an account of the last of these cultures. It was received Nov. 15th, '91, in bouillon and had stood in the culture for ten days.

"Upon opening the paraffin the odor characteristic of tetanus was given off, and a microscopical examination revealed an abundance of bacteria and spores."

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Archiv für Anatomie. 1870.

On the Electrical Irritability of the Cerebrum. By G. Fritsch and E. Hitzig.

Page 308. "In the first experiment we used unnarcotized animals, dogs, but later narcotized, and proceeded to open the skull in as level a spot as possible. Then with the sharp, round bone forceps we removed either the entire half of the skull, or only the part covering the frontal lobe of the brain.

In most cases after experimenting on one hemisphere, we removed the other half of the skull in exactly the same way. In all these cases, after one dog had died of hemorrhage through a small injury to the longitudinal sinus, we left a long bridge of bone to protect it.

Now the dura which had been left intact thus far was lightly cut and grasped with the forceps and laid back to the edge of the skull. Hereupon the dog expressed violent pain by whining and characteristic twitchings.

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But later when it had been exposed to the air for a longer time, the remainder of the dura mater was rendered far more sensitive, a circumstance which, in carrying out the experiment, had to be taken carefully into consideration. However we could shock, in any degree, the pia through mechanical or any other irritation, without the animal manifesting sensation."

After giving a description of the electrical apparatus used, the authors continue: "The following are the results which we give as a summary of a very great number of experiments on the brain of the dog, which harmonize for the most part to the minutest detail, without describing all the experiments.

A part of the convexity of the cerebrum of the dog is motor, and another part is not motor. The motor part is placed, as it is generally expressed, more to the front, the non-motor lies toward the back.

Through the electric stimuli of the motor part, one obtains combined muscle contraction of the opposite half of the body."

(44)

Ferrier. Functions of the Brain. 1886, p. xxii.

APPENDIX.

The Brain of a Suicide.

As stated upon p. 25, by vote of the Congress, Dr. B. G. Wilder was requested to prepare an account of the brain of a dentist who committed suicide, for publication in the Transactions. The following letter explains the absence of the article.

Ithaca, N. Y., Oct. 16, 1894.

Dr. W. H. Carmalt, *Sec'y.*

Dear Sir:—

Referring to the honor done me by the Congress in the action recorded on p. 25, I deeply regret that regular duties and other engagements have made it impossible for me to complete the description of the suicide's brain. An abstract of the paper presented to the American Neurological Association will appear in its *Transactions* and in the *Journal of Nervous and Mental Disease* for . . . 1894. A full account, with figures, will probably be published in the *Journal of Comparative Neurology* during 1895.

Very respectfully,

Your obedient servant,

BURT G. WILDER.

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